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[redacted]RIENCES OF A WOOLWICH PROFESSOR.

EXPERIENCES

OF A

OLWICH PROFESSOR

DURING

fteen Years at the Royal Military Academy.

BY

R-GEN. A. W. DRAYSON, *late* R.A., F.R.A.S.

UTHOR OF "PRACTICAL MILITARY SURVEYING," "THE WOOLWICH CADET"
"AMONG THE ZULUS," ETC.

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PREFACE.

For a period of nearly fifteen years, I was employed as Instructor, and Professor, at the Royal Military Academy, Woolwich. During that time I had a very extended experience, in connection with instruction, and in dealing with various classes of intellects. I also held a second appointment, which was that of instructing officers of all ranks, in Military Surveying, Reconnaissance, and Practical Astronomy. My experiences therefore are unusual, and it is to call attention to these, that I have penned the following pages. Competitive Examinations are now considered the test of intellectual capacity, whilst cramming students for these has become a regular profession. My experience of the results of these proceedings has been very great, and my conclusions differ, in many respects, from those which are generally accepted as correct. Facts are mainly dealt with in this book, theories being dispensed with, and I believe the facts will reveal much that is new to the general reader.

A. W. DRAYSON.

Southsea,
September, 1886.

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EXPERIENCES

OF A

WOOLWICH PROFESSOR.

CHAPTER I.

A valuable Proverb—Value of Stock Knowledge.

It sometimes happens that an immense influence is produced upon our whole life, by a few casual words, or some apparently unimportant incident in our early youth. It was so in my case, when a boy about seven years of age. I was engaged one day in moving some chessmen in accordance with a printed game, that I had found in a book on chess. Whilst so engaged, an old officer who had served through the Peninsular War entered the room, and remarked: "Studying chess, eh? That's right. You may depend on it, that you will always find some use for anything you may learn, however useless such knowledge may seem at the time. That has been my experience through life, and if it is worth while doing anything, it is worth while doing it well."

These were simple common-sense words; but to me at the time they appeared like some of Solomon's pro-

verbs, and they were engraven on my memory, and to a great extent acted on in after life. In these experiences it will be shown how true were the words, and how exactly cause, and effect, were practically demonstrated in my own case.

Having been in delicate health as a boy I was removed from school, and went to stay with an elder brother who was a civil engineer, and who had a considerable amount of work to do in making plans of proposed railways. Whilst with him, I acquired a knowledge of levelling and making sections of country, the use of the theodolite, and surveying in general.

Some time after this I was sent to a Woolwich cram school, and passed into the Royal Military Academy as a cadet.

A few months after obtaining my commission, I was ordered to take charge of a detachment, and to embark at Gravesend in a sailing ship, which was to convey me to Cape Town. The only other passengers were a Lieutenant of Royal Engineers, an assistant-surgeon, and a young officer of the Commissariat. None of these passengers cared to play chess, draughts, or any game of cards, so the time would have passed slowly, had there been no other subjects for amusement, because to always be reading, on board, is wearying. Having devoted about a week to learning what landsmen call the "ropes" on board the ship, my next occupation was to learn from the captain how to find the latitude and longitude. As our voyage lasted seventy-nine days, and

there was nothing special to distract one's attention, I was, before reaching the Cape, able to find my latitude by the sun, or a star, my longitude by a single altitude of the sun, the Greenwich time by a lunar observation, and also the names of nearly all the stars of the first, or second magnitude. What use such knowledge would be to me as an Artillery officer, was not at the time apparent, but the remark of my old friend the Peninsular officer was verified to the letter.

Having been appointed Adjutant of the 3rd Battalion Royal Artillery at Woolwich, I held this appointment during rather more than two years. I was then near my promotion to first captain, corresponding to major in the present day. This promotion would deprive me of my appointment as adjutant, and I should have to proceed to any station at which the officer might be located by whom I was promoted. If the promotions went in regular order, I found that either Bermuda, or Jamaica, would be my destination, and neither of these would at that date have suited me. As I drew nearer to my promotion, I became anxious as regards the future, and saw no escape from a long service in a disagreeable climate, and in an out-of-the-way locality.

On leaving my office one morning, I met near the door an officer who held an appointment as Assistant-Instructor in Surveying at the Royal Military Academy. On asking this officer what he was doing at the barracks and in uniform, he told me that for particular reasons he had resigned his appointment at the Academy, and had

come back to regimental duty. To my inquiry as to who was to succeed him, he said no one had yet been appointed; an Artillery officer would be preferred, but no one seemed qualified to at once take up the duties, as it required a knowledge of surveying, sketching, contouring, and some knowledge of practical astronomy, such as finding latitude and longitude.

Mounting my horse, I rode to the Royal Military Academy and interviewed the Lieutenant-Governor. Fortunately, as a cadet I had been first in mathematics, and had been recommended for the surveying and plan-drawing prizes. I was, however, required to give some proof of my present knowledge, and was directed to make three surveys: a triangulation with the theodolite, a sketch with the compass, and a survey with the chain. Three days were devoted to this work, and the plans were sent in for criticism. Two days afterwards I received a note from the Inspector at the Academy, informing me that I had been selected for the appointment of Assistant-Instructor at the Royal Military Academy. The use of having acquired a knowledge of surveying as a boy, and of practical astronomy on my voyage to the Cape, fully proved the wisdom of my old friend the Peninsular veteran, as I at once obtained a good appointment, and held this during fifteen years. I cannot impress too deeply on my young readers the importance of this principle, because this is not the only example I could quote of similar results having occurred during my experiences.

CHAPTER II.

Cadets — Lecturing — To be Acquired — Questions and Answers — Examples of Questions—Mental Training.

At the time I was appointed as Instructor, the cadets were divided as follows. There were four classes at the Academy on the Common—the fourth, or junior class, the third, second, and first class. The practical class, as it was termed, consisted of the senior cadets, who would in a few months obtain their commissions. These cadets, numbering about forty, were quartered at what were termed the Sappers' Barracks, situated near Green's End, Woolwich. The barracks were dirty, and the situation, in a low part of Woolwich, most unsuitable. There was no ground belonging to these barracks on which cadets could play cricket or take any exercise; but as there was no accommodation at the Academy for this practical class, these barracks had been used for cadets. When I was a cadet, the practical class were quartered in the Arsenal, in a much more suitable position. The digging-ground, where we threw up our field-works, and made gabions and fascines, was just in front of our barracks, and the

rooms were far better than those in the Sappers' Barracks. It was a judicious, and almost necessary alteration, to add two wings to the Academy in order to supply accommodation for the whole of the cadets, as the situation on the Common is far more healthy, and the means by which cadets are kept in hand were more available.

My attendances at the Academy consisted of Thursday afternoon, the whole of Friday, and on Saturdays from 8 a.m. to 1 p.m. During the first few days I was practically only an assistant. I had never given a lecture in my life, and felt somewhat nervous when called upon to make an after-dinner speech; and this was not without due cause, because when I had attempted to make a speech, I realised at the end of it that I had talked nonsense, had not expressed the thoughts that were in my mind, and, in fact, had what is commonly called made "a hash of it." To stand up before forty cadets, and lecture for an hour, seemed to me rather a formidable ordeal, because I did not know exactly where to begin with my subject. My senior in the department was Captain Binney, Royal Engineers, who had long been Instructor to the cadets, and who was an able and clear lecturer. I listened attentively to his lectures, and made notes, and in the course of a week or so ventured on my first lecture.

At the commencement I floundered about, and had to correct myself several times, but in a few minutes, becoming engrossed in my subject, it seemed as though

the words came without exertion, and no sooner had I decided on the particular problem to be explained, than the words to demonstrate, and make this problem clear, fell in like drilled soldiers, and marched on without confusion. I have heard it remarked that lecturing is a gift; I do not agree with this theory. Given the power of voice, and a knowledge of the subject, and lecturing is to be acquired. I have heard many lectures, and when listening to some of these, I have been convinced that the lecturer has had only a superficial, or book knowledge of his subject, and consequently his words would not, and could not, flow with the ease, which is the result of practical knowledge on the part of the lecturer. My proof of a lecture being good or otherwise, is to take a person of fair intelligence to hear a lecturer speak on a subject, on which his hearer is ignorant, then inquire of this hearer how much more he knows of this subject after the lecture, than he knew before he attended it. If he has carried away with him a clear comprehension of the subject, the lecture has been good; if he quits the lecture-hall only puzzled and confused, the lecture has been bad.

When listening to lectures on various subjects, I have too often been convinced that the object of the lecturer was not so much to instruct the audience, as to impress them with the profound knowledge of the speaker. This is to the initiated a clear indication that the lecturer is very closely allied to an impostor, who "by pomposity of speech striveth to be counted wise." This is on a par

with the proceedings of some writers, who not clearly comprehending the subject on which their pen is occupied, assert that the subject is of so complicated a nature, that few minds are capable of understanding it. Lectures are either for instruction or amusement. If for instruction, the lecture cannot be delivered too slowly and distinctly. Repetition in such a lecture is not only allowable, but is usually necessary, when any particular subject has to be thoroughly impressed upon, and absorbed into the minds of, the hearers. The use of technical terms should be avoided if possible in a lecture, unless it is known to a certainty that the students are thoroughly acquainted with these terms. It is not unusual for lecturers to commit the error of supposing, that their hearers are acquainted with the elementary terms and definitions of a subject, which to the lecturer, seem as simple as the use of such terms as round, or square. I remember once causing a whole class of cadets to look aghast, when I announced to them, that I was going to give them a lecture on the method of finding the latitude, by the upper, and lower culmination of a circumpolar star. The helpless appearance of some of the cadets as they sat waiting with pen in hand to take notes was amusing. When, however, I stated that the upper and lower culmination of a circumpolar star meant the altitude of a star when it was above the pole, and then the altitude of the star when twelve hours afterwards it was below the pole, there was no difficulty in comprehending what was meant.

At the examination given by examiners not connected with the Academy, this question was put, and in the form given above, and was answered correctly by nearly every cadet.

After a few months, lecturing became not only easy but pleasant, and I have almost invariably found, that although I might feel unwell when I commenced a lecture, that after an hour's talking, I had got rid of every unpleasant feeling, the mental exercise of giving the lecture seeming to have cleared away all that was out of order in my system. Upon mentioning my experience on one occasion to the late Serjeant Parry, Q.C., he informed me that he had frequently gone into court feeling ill and stupid, that during the first few minutes when he commenced his speech, he could scarcely string together a sentence, but that as he went on, he became clearer and felt better, and when he left off, he was in perfect health, and his brain as clear as it could be.

The change from the duties of adjutant to those of lecturer at the Academy, was very great, but having once overcome the difficulties of commencement, matters proceeded with the greatest ease. Without any special gift for lecturing, practice and a knowledge of the subject enabled me to give lectures at various times and at various places, and from the requests which have invariably been made for me to lecture again at the same places, I am disposed to believe that these lectures were popular. In many towns in England, in India, and in Canada, I have given altogether more than two hundred

lectures, invariably gratis; yet before I commenced my duties with the cadets, I considered it as probable that I should walk on a rope like Blondin, as be able to give the simplest lecture. Practice, however, was the means by which this was accomplished, so that no one can tell of what he is capable, until he has been placed in a position where he is compelled to act. I mention this, because many of my friends possess knowledge on various subjects, of which other people know nothing, and lectures on these subjects, would be interesting and instructive, but when I have suggested to these friends a lecture, they have replied: "A lecture! Why, I could as easily fly, as give a lecture."

If any person is desirous of testing whether he is thoroughly acquainted with any subject, he could not do better than adopt the method I practised with cadets. The lecture was to last an hour, and was to be divided into two parts. First, half-an-hour's lecture; then the cadets were desired to ask questions on any part of this half-hour's lecture, which they did not understand. If these questions were not numerous, then I would ask each cadet a question relative to the subject, and by this means could discover the amount of knowledge which had been absorbed. The second half of the lecture was then given, and the same system of questions and answers adopted. There was a fair amount of mental exertion requisite to give clear explanations of the various obscure points, and these lectures were very different things from the usual platform lectures, given

to the public. In the latter a lecturer may go on for an hour or two, merely giving the conclusions accepted as correct by "authorities," and making dogmatic assertions, just as if he were in a pulpit. Any such request as that of giving additional explanation on any point, would of course be out of the question, and the lecturer is entirely in the dark, as regards the amount of information carried away by his audience.

The questions asked by cadets must be answered clearly, and fully. Unless they be so answered, the Instructor would soon lose caste and be considered inefficient. There were one or two Instructors at Woolwich, from whom, it was said, cadets could not learn. These gentlemen were really too clever. In order to teach, it is necessary that the lecturer should identify himself with his hearers, realise the difficulties which the learner encounters, and then remove them.

I will suppose that a lecture is being given on measuring altitudes of celestial bodies, and correcting these altitudes for parallax and refraction. The correction for parallax, is to be added to the measured altitude; the correction for refraction is to be subtracted. The refraction, again, has to be corrected for the height of the barometer, and thermometer. Having explained these terms to a class, and the method of making the corrections, several cadets would probably ask the following questions:

"Why is the refraction less when the thermometer is high, than when it is low?"

"The density of the atmosphere is the cause of refraction, is it not?"

"Yes, sir."

"Then as heat expands the air, the greater the heat the more expanded the air, therefore the less dense the air."

"Yes; I see that."

"Then the higher the thermometer the less dense the air, consequently the less the refraction."

"Of course it must; I understand it now."

Again another question:

"Why is the refraction greater when the barometer is high, than when it is low?"

"The higher you ascend, the lower the barometer; is it not so?"

"Certainly."

"Then the pressure of the atmosphere causes the barometer to rise."

"Yes."

"Then the higher the barometer the greater the density of the air, and the greater the density of the air the greater the refraction."

"Of course it must be; I see it plainly now."

These and similar questions would be asked probably during half-an-hour or an hour, and would keep the Instructor's brain, in great activity.

The cadets who formed these classes were far above the average of young men in ability. They were the picked men from various schools, and colleges, who had

been successful at the competitive examinations for entrance to the Academy. They had, after this examination, passed two years or more at the Academy, and had then joined the practical class. There was no chance of any Professor, or Instructor, who was not thoroughly acquainted with his work, concealing his want of knowledge from the cadets. They were quick enough to find it out at once, and when they lost confidence in an Instructor, they would rarely ask him for an explanation on any subject, if they really wanted to learn, but would ask many questions if they merely wished to puzzle him. I have heard such questions as the following asked, one after the other, of an Instructor:

"Please, sir, I don't see why, if you reverse the level of the telescope of a theodolite, you correct only half the error that is manifested."

"Why is it that the parallax of the sextant prevents angles to near objects being measured accurately?"

"When you measure the altitude of the sun by means of an artificial horizon, why do you make the two images of the sun tangential instead of coincident with each other? Can you show me this with a diagram?"

"I don't see clearly why the graduated arc of the sextant, should have in it double the number of degrees due to the arc."

"I don't understand how to find my position on a plan; by interpolating from two known objects."

"Will you explain to me why it is that if you

measure with the sextant, the angle between two objects not on the same level, you do not get the true horizontal angle?"

"If I were making a sketch, by aid of the compass, in a strange country, how could I find whether the compass was set wrong by the local attraction of iron?"

"I find I take 115 paces to 100 yards. How am I to make a scale of paces, so as to make my sketch six inches to the mile? I don't see the proportion."

"I made a sketch yesterday of the outline of the Repository, and when I worked round to the starting-point I found I was 200 paces wrong somewhere; the sketch would not join. Can you tell me where I made the mistake?"

"I have worked out this question about finding the local time, and have gone over the working several times, and cannot discover an error, yet the answer comes more than two hours wrong. Can you tell me where my error is?"

"What is the difference between sun time, and mean time, and clock before sun? I can't understand why there is a difference. Can you show me with a diagram?"

"I can't get the contours of my sketch to fit in with those of Mr. S——, who is doing the ground next mine; his contours don't agree with mine. Can you tell me which of us is wrong?"

"When getting the distance of a ship at sea from an

elevated battery, why is it necessary to know whether it is high, or low tide?"

Many of these questions are very simple, and readily answered; others require long explanations, and clear diagrams. Some again require an accurate knowledge of every detail of the country, which has been sketched, or contoured, and in some instances the particular class of error, that will be made by the cadet in certain problems, such for example as writing down the tangent of an angle instead of the cosine, when looking out these from his book of Logarithms. Thus in an instant the error may be hit off, and the cadet, not being aware that such errors are repeated, and have been repeated, by particular types of mind, is puzzled to know how it is, that a mistake which he failed to discover, after half-an-hour's investigation, is at once, and at sight, seen by his Instructor.

Any person not well up in his subject, would soon be puzzled by the repetition of such questions. During perhaps half-an-hour, he might get on, and be able to explain in a satisfactory manner, the problems required to be made clear; but sometimes these questions would be asked, during every minute of a three hours' attendance, and to carry out such work ably, renders it necessary to keep the head very clear.

There is, however, no work more satisfactory than this. To find some thirty or forty intelligent young men, who, when you first meet them, are entirely ignorant of a subject, gradually growing intellectually under your

instruction, becoming well acquainted with the principles of the science, and at length proving by practice, and examination, that they have no mere sketchy knowledge only, but a thorough grounding of information, is to my mind, an agreeable recreation more than a labour. Especially is this the case, when the subjects taught, are not merely elementary questions, but are those requiring a high-class previous training.

CHAPTER III.

Professors at the Academy—Professor Bradley—Cost of a Scientific Truth—Sketching from Nature—Major Gore—The Art of Teaching—Capacity to Teach.

Upon my joining the Royal Military Academy as Instructor, there were at the theoretical or junior portion of the establishment, several Professors, and Instructors, who had occupied their positions at the time I was a cadet, and who had been my Instructors. First among these, to whom I will refer, is Mr. Peter Bradley, because he had been the longest time at the Academy, and because he was a man of very great ability. The subject taught by Mr. Bradley was geometrical drawing. When I was a cadet, this geometrical drawing was not popular among the students, and it was irreverently termed "Peter's stuff." It was not until I had left the Academy, and worked out various problems by myself, that I discovered the value of practical geometry, and the certainty of the accuracy of this science. Instead of having, as is often the case, a number of vague problems based upon a mere vague theory, and called by its superstitious adherents "exact science," we have in practical geometry, every item as sound, and as true, as

that two sides of a triangle are greater than the third. If practical geometry had been understood in ancient times, we should never have heard of the earth being a flat surface, and if this same science were now understood, we should not hear so much nonsense, and such vague theories put forward, by some of our popular or platform lecturers as facts, whilst accurate geometrical proofs are ignored by such men, principally because they are ignorant of geometry. Mr. Bradley, in his particular science, was certainly at the top of the tree, but he was not very successful as an Instructor, mainly, I believe, because he knew too much, and could not mentally come down to the minds, to which he had to give instruction. He failed to realise, how it could be, that any individual, gifted with common intelligence, could fail to see at a glance, the solution of problems, which he submitted to them. The cadets finding difficulties in the very commencement, of the attempted solution of the question, and failing to get a clear explanation, would shortly give up any hopes of thoroughly understanding this science, and would not care to bring to bear on it, the brain-power at their disposal. When I joined the Academy as Instructor, there were many problems, to which I had given long and careful consideration, but the accuracy of which I doubted. Mr. Bradley undertook to examine some of these for me. After some days' consideration, he informed me that one of these problems, which is even now accepted as accurate and true by the orthodox men of science, started with a geometrical impossibility as

absurd, as stating that the centre of a circle varies its distance from the circumference, but yet always remains the centre of the same circle.

Mr. Bradley was very severe on those whom he termed the "quacks of science," viz., men who by the mere result of seniority, had arrived at the head of certain scientific societies, or held positions which caused the general public, to accept the mere assertion, or opinion, of these men as conclusive. He frequently remarked that the priestcraft of a former age, was being repeated in modern times, under the name of science, and he gave me evidence of the absurdly incorrect conclusions arrived at, by a well-known scientific authority, on a question which had been officially submitted to him, but which he evidently had entirely misunderstood.

It was not long before I was able to gratify Mr. Bradley by showing him a letter I had received from one of the highest scientific authorities of the day, conveying his opinion on a question, I had submitted to him. It was relative to the effect produced by the pitching of a ship, when paying out a telegraph cable. I claimed, that when a very light cable was being rolled out from a ship, this cable instead of sinking almost vertically, would form a very acute angle with the surface of the water, and might perhaps if the deck of the ship was very high out of the water, touch the water fifty yards from the ship. The rise and fall of the ship would therefore bring very little strain on this cable. If, however, a cable was very heavy, it would descend into

the sea almost vertically, and, when the ship pitched and the stern rose, she would in the rise, have to lift the whole length of the cable, which had not reached the bed of the sea. I gave, as an additional example, the well-known fact that a vessel in a heavy sea, anchors and pays out, as much cable as she can spare, the pitching of the ship then being less likely to cause the cable to part, than if she were anchored with only a short cable. To my astonishment I was assured by the scientific authority, that I was quite wrong, for that he had carefully worked out the question, and had found that the heavier the cable, the less would be the strain on this cable, caused by the pitching of the ship, and this reply was signed with a name which for years had been looked upon as belonging to so profound a scientist, as to cause people to be termed heretics, who even questioned his infallibility. Upon submitting the same question to Mr. Bradley, he, after a few minutes' examination, gave me a geometrical diagram, showing that the lighter the cable the less would be the strain produced by the pitching of the ship. I then informed him that the very opposite result would occur, according to the opinion of another gentleman.

"Whoever says so," replied Mr. Bradley, "must be utterly ignorant of the elements of geometry and mechanics."

I then showed him the letter I received, and when he had seen the signature of the writer, he said: "'Now you can understand the types of mind, of the learned

Professors of Padua, who announced that Galileo's theory of the rotation of the earth on its axis, was the emanation of the feeble brain of a lunatic or heretic."

Mr. Bradley was also somewhat hard on what he called the "showmen of science," and would remark, that the fuss and trumpeting, and money spent on certain expeditions, were merely for the purpose of parading before the public. When expensive expeditions were fitted out, to proceed to distant parts of the world to observe an eclipse of the sun, and after the illustrated papers had given portraits of those gentlemen so employed, standing beside large telescopes, he would say: "Now let us see what important fact they will discover, and make known to us, and let us put this fact beside the cost of the expedition, and we can then estimate how much it costs to learn a scientific truth. If, however, as was too often the case, nothing new or important, was discovered, and the history of this scientific expedition consisted in a description of the voyage, and how the observers set up, and adjusted their instruments, and nothing more, he would boldly assert, that there was more humbug in what was called science, than in any other subject in the present day."

Mr. Bradley, after many years at the Academy, was taken suddenly ill, and died in a few days. He was a man of rare genius, and the subject of practical geometry, owes more to him than to any man living.

He may have many followers and imitators, but scarcely an equal on this special science.

Another civilian Instructor at the Academy who had occupied his position when I was a cadet, was Mr. G. Campion. The style of drawing taught by Mr. Campion was simple but effective. He was great at form and perspective, and very chary about using colour. His idea was that colour required years of study, before it could be used by amateurs, to make anything but a daub. He confined his pupils to three or four colours only, French blue, lake, burnt sienna, and lamp black. He had a perfect horror of Prussian blue, gamboge, and other brilliant colours in which one of his rival Instructors indulged. Mr. Campion's teaching was good. The "dodges" of art, as it may be termed, were familiar to him, and his sketches most effective. When I joined the staff of the Royal Military Academy, I had several rough sketches, that I had taken in Africa; these I submitted to Mr. Campion, who would make from them an artistic sketch, showing me what to leave out, and what additions to put in. Sometimes, however, these additions, although adding to the effect, were made with a disregard to truth. And on one occasion I had the best of the criticism with Mr. Campion. He showed me a sketch, he had completed of a scene in India, made from the rough sketch and descriptions given him by an officer. On looking at this sketch, I ventured to say that I thought it was not quite accurate to nature. "It is quite accurate," said Mr.

Campion, "the elephant which I added, to give the character of India, I took from a photograph of an Indian elephant, all the remainder is from the original sketch."

"But the elephant's tusks," I remarked, "were they in the photograph?"

"Well, no, they were not. I added these for effect, the white of the tusks being required as a relief to the dark in the sketch."

"It is unfortunate," I replied, "that you have put the tusks in the wrong jaw."

"Surely not, I have put them in the lower jaw, and pointing slightly upwards."

"And Nature places them in the upper jaw, and pointing slightly downwards."

For an officer, or amateur, who has but slight time at his disposal for making sketches, Mr. Campion's style was admirable. An accurate outline could be taken, distances marked, and the individuality of the sketch indicated by his system, more rapidly and more correctly than by any other that I have seen. The mere daubing on of colours, a proceeding of which some amateurs are so fond, was discouraged in every way by him. Aaron Penley was the other Instructor in Drawing, and was great at water-colours. He was a very small man, but very important, in his manner. There was a great deal of rivalry between Mr. Campion and Aaron Penley, each smilingly looking with some pity on the system of drawing taught

by the other. Mr. Penley had joined the Academy after I had left it as a cadet, so that I made his acquaintance only when I joined the staff of the Academy.

Mr. Lovey, and Mr. Feiling, had been my Instructors when I was a schoolboy preparing for Woolwich, and afterwards when I was a cadet; and I found them Professors of the two languages, French and German, when I became Instructor.

Major Gore, an officer who had retired from the Royal Artillery on full pay, had joined the Academy as Instructor in Plan Drawing when I was a cadet, and he held the same position when I became Instructor in Surveying to the senior class. I have always considered that the red-tape regulations bore very hardly on this officer. By his long service as an officer of Artillery, he was allowed to retire on full pay; he may thus be said to have earned this full pay, in consequence of his service in the regiment. He then took the appointment of Instructor in Plan Drawing, and having served in this appointment a sufficient number of years, to entitle him to a pension, had he been a civilian, he applied for retirement, expecting to obtain the pension for his long service at the Academy. By some regulation, however, made to bear on his case, it appeared that he could only receive a certain sum as pension, which sum he already drew as a retired officer; so that nothing farther could be given him, because he was an officer, for his many years' labour

at the Academy. The injustice of such a regulation is evident, but the letter of the law was rigidly carried out in his case; and so, because he had gained a pension for his services as an officer, he was allowed no pension for his services at the Academy, whereas, had he been a civilian, these same services would have entitled him to a pension of about £220 per annum.

Although there were no actual proceedings as regards senior and junior, yet Major Gore was Instructor in Plan Drawing to the junior cadets, whilst I, who had been a cadet when Major Gore was an Instructor, taught the senior cadets, and any defects, or supposed defects, in the teaching of the juniors, was remarked on by us in the senior class. I always felt that this position was unpleasant, that two comparatively young officers should hold positions senior, as it were, to an officer who had been commissioned whilst we were learning our multiplication table.

It is impossible to prevent some Instructors being popular, and others unpopular, among cadets, and it sometimes occurs that an Instructor, or Professor, may be popular with one class, and unpopular with the next. Major Gore, however, from his bearing and treatment of cadets, could not help being popular with all, and much regret was felt, when he retired from the Academy, and sympathy for the injustice, the regulations caused him to suffer.

The manner in which cadets had to be dealt with was peculiar. They were young men, and could not be

treated as schoolboys, yet they were under strict military discipline. Many of them were thoroughly sound mathematicians, knowing as much, if not more mathematics than their teachers. The work in my department consisted mainly in pointing out how their mathematical knowledge could be applied, to the special subject with which we dealt. Hence, when we had pointed out how the three sides of a spherical triangle could be obtained, for finding the local time, the problem was practically known, as each cadet could obtain any given angle, when he knew the three sides of the triangle. In giving instruction on the application of stock knowledge, Professors differ very much. I knew one individual who was, perhaps, one of the best mathematicians in Europe, but who had not the gift of imparting knowledge. It appeared as though he could not descend from the mental pedestal, on which he stood, to make himself intelligible to a less profound intellect. Consequently, if a cadet encountered a difficulty, this gentleman would too often make the difficulty greater, instead of less, by the complex explanation he would give. It was the same when I was a cadet. Some of my teachers had not the knack of clearing away difficulties or of realising the state of mind of the pupil who encountered the difficulty. One of my Instructors, however, was an exception to this defect, viz., the late Professor Barlow, who was Instructor in Mathematics when I was a cadet. When a difficulty was puzzling one, a question asked of this Professor, would cause him,

to reflect a few moments, as though he were reading one's mind. Sketching out a diagram, and giving a few words of explanation, he would at once make simple, and intelligible, all that was previously complex, or obscure; and, in addition, he seemed to take a delight in giving these explanations, without exhibiting any of those signs of irritation, which are sometimes shown, when rather stupid questions are asked.

The capacity for teaching, is not in proportion to the amount of knowledge possessed by the teacher. Self-taught men I have usually found make the best instructors, unless they act as mere copies of an able teacher from whom they have learnt. When preparing for my entrance examination to the Royal Military Academy as a cadet, I was nearly a twelvemonth at one of the cram schools at Woolwich. I knew as far as rule of three when I joined the school. During twelve months I was taken through fractions, decimals, cube-root, and addition in algebra; but so obscure was the system of teaching, and so convinced was the schoolmaster, that if a boy did not comprehend a problem, the way to make it clear to him, was to cut him across the hand with a whalebone, that clear explanations were rarely attempted. I had then three months remaining, during which I had to learn multiplication and division in algebra, simple equations, quadratic equations, and cubic equations, also the third and part of the fifth book in Euclid. By the cram school system, it was considered impossible to impart a knowledge of these subjects in less time than

from twelve to eighteen months. I was therefore given up as a failure. Fortunately I was transferred to the care of the Rev. Thomas Grose, who lived in Trinity Square, Tower Hill. This gentleman was one of the clearest headed, and most able Instructors, I have ever met. Instead of the whalebone, or cane, he used diagrams, and clear explanations when he found his pupil in a difficulty. Instead of losing his temper, and abusing his pupil for stupidity, he seemed to pour the oil of his intellect on the troubled brain, and to calm it, and cause it to reflect. During the first week my previous instruction was tested, and where found weak was strengthened. During the next week, quadratic equations were attacked, and by the end of the week following were mastered. At the end of two months cubic equations were my familiar friends, whilst the second, third, and fifth books of Euclid, were a kind of pleasant reading. The remaining month was devoted to solving problems in the subjects I had learnt, and my examination for entrance at the Academy was pronounced very satisfactory. To me I found it more than satisfactory, for several of my former schoolfellows, at the cram school, who had been held up to me as patterns of intelligence, and who had been in the first class whilst I remained in the last, failed to pass at the same examination as that, in which I had been successful. These facts are mentioned, because I do not think sufficient importance is attached in the present day to the science of teaching, or imparting knowledge.

Men are usually selected as Instructors on account of what they know, not on account of their capacity for imparting what they know; for their power of imparting is never tested until they are appointed, and there is no means known, or practised at present, by which any individual's talent, or genius, in this line can be readily gauged. Give the same examination paper to two individuals, A and B, as a test of their fitness as an Instructor, A might obtain more marks than B, yet B might be the better Instructor; the actual fact of having passed a better examination, not necessarily demonstrating that the powers for instructing were better. When I refer to examinations I must offer some more remarks on this very important question.

CHAPTER IV.

Academy Regulations—Discipline—Military Instructors—Schoolboy Officers—At Mess—Artillery Studies—Unreasonable Regulations.

THE regulations and interior economy of the Academy, at the time to which I refer, were defective in many ways. The cadets were youths specially trained for military service, and as much of the military element as possible was required to be instilled into the pupils. The Instructors at the Academy connected with military surveying, fortification, and artillery were military men. Those connected with landscape drawing, geometrical drawing, mathematics, and languages were civilians. There was no limit to the time of employment of the civilians, as long as they did not indicate actual decrepitude, or mental imbecility. Consequently, as before stated, there were several civilian Instructors at the Academy, who had occupied the same position during many years. The discipline in the Academy was maintained by a corporal (of cadets), whose duty consisted in preserving order, and granting permission to cadets to leave the room.

The Instructor had nothing to do with the discipline; but if he found any noise or disturbance going on, he called the attention of the corporal on duty to this, and the corporal endeavoured to preserve order. Some corporals were efficient, and did their duty well; others were feeble, and never kept a class in proper order. The system of having a cadet corporal to keep order, was supposed to leave the Instructor free to give instruction; and as civilians are not always experienced in the system of maintaining discipline, and had really no military authority over the cadets, the corporal on duty was a necessity. After many years of this system, an alteration was made, by giving to the Instructors the authority and responsibility of maintaining order. The efficiency or otherwise of the master, as regards preserving order, could then be easily ascertained by the state of his class. In Lord Malmesbury's memoir lately published mention is made of a class of students, who used to indulge in slowly pushing a large table against their lecturer, until he was jammed in a corner of the room, and could not move. There was a civilian master once at the Academy, who was of a timid nature, and who, when he gave marks for questions, which the cadets considered inadequate, used to be subjected to the following experience.

A cadet with a sharp pair of compasses would go to the table in the octagon, where this master sat, and would say:

"I think, sir, I have been treated very unfairly."

This remark was followed by the cadet digging his compasses into the table near the hand of the master.

"I ought to receive many more marks."

Another furious dig of the compasses.

"Your marks Mr. ——, are, I consider, quite sufficient," would be the reply of the master.

"I have been treated very unfairly, sir, and think it a great shame I have been given so few marks. I ought to have several more."

Another demonstration with the compasses.

"Well, Mr. ——, I will look over the question again, and see whether I can give you a few more."

Such a proceeding as either this or the former, could not be practised on a military Instructor. The officer accustomed to maintain discipline, would check the slightest signs of such irregularities, before they arrived at such a state; and, in fact, there must be something very feeble in the character of the Instructor, for such things to be even thought of; and as the cadets were in nearly every instance young men with correct ideas, and were gentlemen, the style and manner of the Instructor must be such as to invite these liberties.

Civilian Instructors were more expensive to the Government than military Instructors. Civilian Instructors after a certain number of years of employment, could claim a pension; military Instructors, however, after attaining the rank of lieutenant-colonel, returned to regimental duty, and received no pension. Consequently if all the Instructors at the Academy were military men,

there would be no pension list connected with that institution. The most important item, however, connected with such an establishment as Woolwich is, who are the men most fitted to instruct young men in the various subjects required by an officer? For languages such as French and German, natives of the two countries are essential. It rarely occurs that an Englishman can speak a foreign language as well as a native of the foreign country, and if an English officer did exist who could do so, it would be difficult, if not impossible, to supply his place when his period of service had expired. There are, in both the Royal Artillery, and the Royal Engineers, officers who are able mathematicians, and who could teach all that was required in this science; there is also an advantage in having officers to instruct, because these officers have practically found what branches of mathematics have been most useful to them in their profession, and they could enlarge on these, and curtail those portions which rarely, if ever, are required by officers. There is also, I believe, less chance of an officer, who has knocked about the world, and has frequently been left to his own resources, running in a groove and mistaking the means for the end, than there is of a man of one subject only, doing so. The man who is a mathematician, and nothing else, too often looks on mathematics as the end, and the all of everything. The application of simple mathematics to practical purposes is sometimes beneath the notice of so vast a mind, and we may get overrun by mere theorists, instead of having

practical men who can do what is required at the right time. I can see no reason why military men, accustomed to discipline, acquainted with the practical application of mathematics, and with a thorough knowledge of the science, should not be employed as Instructors in mathematics at the Academy.

Fortification, artillery, and military drawing necessitate the employment of officers. The changes which have been introduced at the Academy since I first joined it as Instructor, have been in the direction that I have named. Formerly, every mathematical Instructor was a civilian; the Instructors in military and landscape drawing were civilians, as also those who taught languages. At present there are only nine civilian Instructors, whilst there are sixteen military Professors or Instructors. The nine civilians are disposed as follows: Three for mathematics, two for French, two for German, one for landscape drawing, and one for chemistry and physics.

It was during the time that I was employed at the Academy that a system was introduced, the advantages of which are, I consider, very doubtful.

A cadet previous to his joining the Academy as a successful candidate, has endured a long and severe training in some school or college. He passes through the Academy, having succeeded at his various examinations, and obtains his commission. On joining the Artillery, he is again sent to school, and remains some months studying in the Department of Artillery Studies, before he is sent to perform his

regimental duties. This system presupposes one or two facts: First, that the training given at the Academy, does not fit a cadet to at once take up his duties as an officer, but renders it necessary to place him under further instruction, before he is qualified to do so; secondly, the starting of such an establishment for young officers, assumes that there is no person at the Academy qualified to give the instruction which is given in the Department of Artillery Studies. My reason for making the latter statement is, that economy is one great object of every Government, and economy in this system is put on one side, and for what reason, other than that stated in my first remark. A cadet is paid for by his relatives. Let us suppose £80 a year is paid for a cadet at the Academy by his relatives. His cost to the country whilst under instruction is not considerable. He is then commissioned, and is paid by the country; yet whilst so paid, he is practically at school, and is still learning those subjects which are to fit him to be an efficient officer in the future. The difference of cost of the two systems is so considerable, that we must look to the cause of such a system having been adopted. It must be, because the cadet could not learn at the Academy the subjects taught in the Department of Artillery Studies, that this other school has been adopted; or some other reason must be assigned — which reason is not apparent.

When a number of young men have been closely

associated during some two or three years, they usually form themselves into cliques, with set ideas, and too often with cramped views. They start as officers, but are still little more than schoolboys of a large growth. They are herded together, and associate mostly with each other, and feeling the greater liberty which they now enjoy, are sometimes disposed to enjoy this boisterously. The element of seniority does not impress itself much on young officers, who join in numbers varying from twenty to thirty. These numbers alone carry with them a certain force. Formerly when a cadet obtained his commission, he was posted to a company or battery at once, and in some cases did not join at Woolwich. He was then immediately separated from his former companions, and brought into association with senior officers, whose influence he felt, because he had no other companions, his character was thus formed earlier, and he felt his responsibilities more than if one of a crowd. When I joined at Woolwich on gaining my commission, the Woolwich mess was one, where veneration was at once called into action. Several old officers then dined at mess, some of these having served through the Peninsular War. They did not possess more than one medal, and some not even one, such slight affairs as those which took place in the Peninsula, finishing with Waterloo, being too unimportant to call for a decoration; consequently such veterans could not compare with the brilliant appearance of those officers who in modern

times have been fortunate enough to be present at such battles as those at the Cape, at Ashanti, and in Egypt, and who are decorated with six or seven medals. Yet the bearing and appearance of these veterans, produced their effect upon young officers, and the mess, although social, did not entirely lose its character of a military parade. Some years ago, when I returned from India, I dined after several years' absence at the Woolwich mess. There was not one officer at the mess besides myself above the rank of captain. It was band night, and several civilian guests were present. As soon as the cloth was removed, the batch of last-joined young officers rushed to the door, and had a race up the stairs to the billiard-room. During the evening, when the band allowed other sounds to be heard, those sitting at the mess table, were favoured with the yells, and banging of cues on the floor of the billiard-room, when the result of "red upon white, yellow your player" had been that red had fluked white, or had sent the red ball off the table into the fireplace. Such a desecration of a room where the heroes of a hundred fights had formerly maintained an order and a dignity which impressed itself on all ranks, was similar in its effects on me, to what most people would feel who found Westminster Abbey turned into a playground for the roughs of London.

Such a scene as that which I have described could not have occurred formerly, when the young officers did not swamp the mess, and the utter absence not only of discipline, but of proper feeling, would soon have been

checked with an iron hand, had even a tenth part of such rowdyism been manifested.

If these youths, whose conduct showed they were but schoolboys, had been kept six months longer under the strict discipline of cadets, and had been taught those subjects which they were now taught as officers, and then had been separated and posted to various batteries at home or abroad, these habits and thoughts would have been entirely changed, and in a few months, if placed under judicious commanding officers, they would have become thoughtful men instead of reckless lads. In regiments of the Line young officers join singly, or in twos or threes at the most. Their conduct, and style, are carefully watched by their seniors, and any defects are quickly pointed out, and usually cured. They have not time to get into bad habits, whereas in the Artillery such habits, not being at once corrected, become to a certain extent chronic, and difficult to eradicate. I know of a case where one of these young officers, who had remained a long time at Woolwich, went up to a lieut.-colonel in the regiment, who, with a civilian guest, was sitting at the upper table at mess, and said: "Would you mind going to that other table, as a lot of us cricketing fellows want to sit together at this table?" The reply of the lieut.-colonel was such that the young subaltern did not probably venture on such a request again; but even the remark to a senior officer indicated the arrogant impertinence of the emancipated schoolboy.

The Department of Artillery Studies was introduced

many years ago, for the purpose of instructing officers in those professional subjects which are being continually introduced into the Royal Artillery. Officers who may have been abroad during several years, and have had no opportunity of keeping up with the rapid improvements of modern times, find, on their return to Woolwich, that they are old-fashioned and behindhand. These defects can be remedied by a course of study in the department named. At first officers did not take kindly to the school, and there seemed a probability of its being a failure. A law, however, was introduced that no appointment to any of the departments was to be given to any officer, who had not passed through the advanced class, as it was termed; and an addition to this law was shortly made, viz., that any officer who did pass through the advanced class could claim an appointment. This latter law was carrying things too far. There are many men who, by pure hard work, and by cramming, can pass examinations in a satisfactory manner, but whose mental capacity to employ common sense, and judgment, is very feeble. There are other men whose whole career has shown them to be men with great and active brain power, but who have not submitted themselves to the mill-work of a school when at a mature age. To give as a right an appointment to the former, and to bar any claims of the latter to special employment, is a mere blind adherence to a rule. This fact was demonstrated when an officer, who, during his previous career, had indicated an absence of common sense and judgment

in almost everything he undertook, joined and passed through the advanced class, and came out nearly at the top of the list. The reasonable conclusion as regards this question is, that any individual, A or B, is better fitted for special employment after he has passed through a course of study, than he was before he did so, but it does not follow that he is better qualified than X or Y, who have not passed through such a course, but whose career has shown that both X and Y are most able men. A curious example of the mere routine system that officials sometimes adopt, was illustrated in my own case. Officers who had passed through the Staff College, or the advanced class, sometimes joined a class under my instruction, where the higher branches of surveying and practical astronomy were taught. Teaching these subjects required a thorough knowledge of mathematics and the application of these, and I frequently found that officers who had passed most brilliantly in mathematics at the Staff College, or advanced class, were quite unable to apply their knowledge to practical use. Thus officers who had passed such examinations as were supposed to qualify them for the Staff, came to me for additional instruction. When my period of service as Professor at the Academy had expired, I was informed that I could not be appointed to any Staff appointment, *because* I had never passed the Staff College. Considering that during many years I had been the Instructor of those who had passed the Staff College, the regulation seemed certainly peculiar. Several of the Instructors

at the Staff College had been trained by me to pass the examinations which had gained them their appointment; consequently, had I joined that establishment, I should have been placing myself under the instruction of my own pupils, in order to prove that I was qualified for Staff appointment.

CHAPTER V.

Practical Astronomy—Science and Hens—Scientific Jobs—Instruction in Astronomy—Variable Intelligence.

As an illustration of the truth of the remarks I have ventured to make in the early pages of this work, relative to the value of acquired knowledge, I will mention the cause of my obtaining the appointment of Instructor in Topographical Drawing and Practical Astronomy to the Royal Artillery, and some incidents connected with my experience whilst so employed.

Holding the appointment of Instructor in Surveying at the Royal Military Academy I had three spare days per week, during which my time was at my own disposal. My work at the Academy was finished on my employed days at 4 p.m., so that every evening was free. At this date an officer of Artillery was selected as one of the commissioners to lay down a parallel of latitude in North America. This officer was remarkable for his ability, and was selected wisely; he had not, however, much acquaintance with transit observations, and was desirous of gaining practical knowledge before starting on his expedition. Where to gain this instruction was the question. There was

no establishment at Woolwich where an officer, desirous of learning such a subject, or even Military Reconnaissance, could be instructed. It was found by, I believe, Colonel Wilford, who was then Governor of the Academy, that an appointment sanctioned by Government had been vacant during some years, no one having been found anxious, or qualified to take it. This appointment was Instructor in Practical Astronomy and Reconnaissance to the Royal Artillery.

Colonel Wilford asked me if I knew enough of the subjects to undertake the appointment, and if I did not know sufficient, whether I was prepared to work it up. At the time I could find my latitude in various ways by the sextant, also my longitude by the sextant, I could also take the necessary observations, and work out a lunar observation, but was unacquainted with the transit instrument. Being acquainted with the theodolite, I soon learned the use of the transit by aid of Loomis's "Practical Astronomy." By the kind aid of the Rev. Robert Main, then Senior Observer at Greenwich Observatory, I was granted permission to take my transit instrument to Greenwich, and placing and adjusting this in the meridian, I commenced my observations. During two years I had the advantage of studying at the Observatory, and went through the course there, from reducing the observations, comparing chronometers, taking transits of various celestial bodies, and, in fact, gathering all the information that

could be gained. I then felt qualified to instruct any officer in the various branches of this subject. I had already taken up my duties as Instructor to the Royal Artillery, in addition to my Academy duties, and the work was sometimes rather hard. A class of officers would be under my instruction, and would be taking observations until after midnight. At 8 a.m. on the following morning it was necessary for me to be in Academy, and to immediately commence a lecture to cadets. The attendance of officers was variable; sometimes seven or eight would be working with me during from three to twelve months, according to the time at their disposal; at other times six months would elapse, and scarcely an officer would join the class. This work with officers was most pleasant, and the rapidity with which most of them acquired a thoroughly practical knowledge of the subjects, was gratifying. Every officer before joining for any Staff appointment, was required to be able to make a contoured sketch of a portion of country, and also a reconnaissance. Many officers came to me to acquire a knowledge of this science, and they then had to pass an examination, Colonel Scott, R.E., being usually the examiner. Out of the numerous officers who were under my instruction, not one failed to pass the examination in a satisfactory manner. Amongst many other officers who studied with me during the time I held this appointment were Sir Frederick Roberts; Colonel Chapman, now Assistant Quartermaster-General in India; the late Brigadier-

General Murray, late Adjutant-General Royal Artillery in India; and many others. Several of these officers acquired such a knowledge of astronomy, that they would have been qualified to take charge of any observatory. The practice one obtained during this instruction was very great. Sometimes from twenty to thirty observations with the sextant would be taken per day, and worked out, to find local time; and during one year I measured and worked out, over 700 lunar observations, the officers studying with me aiding in the details. During the working out of these lunars I discovered a source of error connected with the lunar observation, which is at present unknown to astronomers. It is of too technical a character to be here described, but is a very important and interesting fact.

An amusing incident occurred shortly after I had taken up this appointment. The class of officers studying with me, used to meet at the Royal Artillery Institution, and we occupied one of the rooms, when doing indoor work. On the Common and near the Repository, stands a building termed the Royal Artillery Observatory. This building was built by a subscription obtained from officers of the Royal Artillery, amounting to, I believe, £500, supplemented by a grant from Government. There are several rooms in this Observatory, among them a circular observing-room with a revolving dome. No one having taken any interest in this building during many years, it seemed quite neglected, and believing it to be a very suitable

locality for the class of officers, and being desirous of examining what instruments were in the building, I made inquiries from the then Secretary of the Royal Artillery Institution relative to this Observatory. I then ascertained that two rooms were retained for the Royal Artillery, but that the remainder were given up to a staff of clerks, employed in working out some calculations connected with some magnetic observations. I had frequently noticed on passing the Observatory, that three or four men were lounging about outside the building enjoying a smoke, and I had my suspicions raised, that not only the importance, but the amount of work done at this establishment was greatly exaggerated. I fear that Mr. Bradley's sceptical remarks had infected me; so one day, when in plain clothes, I visited the Observatory, when I saw the men very busy with the fragrant weed. I was met by an old man who asked me what I wanted.

I told him I wished to see the rooms belonging to the Royal Artillery.

He replied that I had no business there, and that the whole place was in his charge.

From a room, which I knew was termed the Prime Vertical Room, I heard noises, which certainly did not belong to science; but I bided my time for a further reconnaissance, and left the building without further inspection.

Taking up a commanding position in the Repository, I noted the departure of the various men to their dinner. I then entered the Observatory grounds, and peeped into

the window of the transit room, which I found occupied by fowls, several nice-looking hens being perched where the transit instrument ought to stand.

It required some months' correspondence, and a great deal of perseverance against obstructions, before I obtained the use of these two rooms. Among other objections it was urged that the noise of any one coming into the Observatory would so disturb the men there employed, that the profound calculations on which they were engaged, could not be carried out, and the most disastrous results to science would ensue, and the discoveries which were being made, might never be given to the world. During several years these men have been employed, but the discoveries have not yet been made public. It might be interesting to inquire what amount of money has been spent on these magnetic calculations, and what actual discoveries have been made; also, what is the practical or theoretical value of the work, that was supposed to have been accomplished by so large a staff during so many years.

Having obtained possession of the two rooms at the Observatory, I found, besides a valuable library of astronomical books, that there were some half-dozen Hadley's sextants, artificial horizons, a portable transit instrument, and a large chronometer. A telescope, about five feet long, with a three-and-a-half inch object-glass, mounted on a rough wooden stand, was the only observing instrument, and was quite unworthy of the Observatory. After many months I succeeded in obtaining, through the

funds of the Institution, an admirable telescope; the object-glass, I think, was eight inches in diameter. This telescope was made by Grubb, and was mounted equatorially, with clockwork movement. The remainder of the rooms at the Observatory were, after two or three years, given up to the purposes for which they were intended, namely, as a place for officers to study; the important magnetic calculations having been finished.

During several years I gave instruction to over one hundred officers in the use of the transit instrument, in the Meridian and Prime Vertical, and I considered that the rooms in which the instruction was given, were more fitly employed, than they were when used as a fowl-house. I know and felt, that I had made myself very unpopular among certain gentlemen who were connected with these magnetic observations, by underrating the importance and scientific value of the work which they were supposed to be carrying out; but as no results have yet appeared, nor seem likely to appear, I feel quite satisfied that what I did, in no way retarded the advance of real science, though it may have interfered with the comfortable sinecures of a sort of Mutual Admiration Society.

Obtaining this appointment was due to the fact, that, when a young subaltern, I had learned how to find my latitude and longitude, and to work out a lunar observation when on a voyage. To have commenced studying practical astronomy, when unacquainted with the mere elements of the subject, would have been a laborious undertaking, and I don't think I should

have ventured to have taken the appointment, had I been thus ignorant of the whole subject; knowing, however, that I was at least two months ahead of any officer who might come to study with me, I believed that by practice and study, I could keep ahead of him, and in consequence of the kind assistance given me at the Royal Observatory, I gained a knowledge of the whole details of the science, which rendered it almost impossible that any of my pupils could possess more information on astronomy than I had managed to gain.

This instruction was of a kind of conversational character, and the desire of these officers to acquire knowledge was so great, that my labours were very light, a few hints being in most cases sufficient to enable them to work up a subject during the next two or three days by themselves. The difference between the intellectual capacity of these officers was very great. Now and then an officer would join the class, who evidently possessed the idea that he knew all about the subject, in consequence of having acquired a smattering at the Staff College, or elsewhere. He would then endeavour to air his knowledge, and would ask a series of questions which he considered would be very puzzling. One individual commenced by taking the Nautical Almanac, and turning over the pages, asked in quick succession the following questions: What is meant by the sun's declination at apparent noon, and then at mean noon, what is the difference?

What is meant by the equation of time? Why does the sun's right ascension vary? Why is it called right, and why ascension? What is the difference between Greenwich meantime, and local meantime? Why does the sun's semidiameter vary? I want to know all these things before I begin.

As the reply to each question would require a diagram, and probably fifteen minutes' explanation, I asked him which he would like explained first; but I suggested that he had better learn the definitions, as I found he did not understand the difference between declination and polar distance. This officer attended the class twice, and then gave it up. Whilst another officer who joined the class at the same time, worked steadily during three months, at the end of which period he measured three lunar observations one evening at intervals of half-an-honr, and worked these out; each observation he took by himself, and the working he accomplished without any aid, and the greatest error did not amount to ten seconds of time. Those persons who have worked at lunars, will appreciate this officer's skill as an observer, and his accuracy as a mathematician.

The experience gained in this instruction, taught that each officer required some variation in the system of teaching. Diagrams and repeated explanations were required by some, whilst others could not comprehend without a model of the problem was made for them. Some officers were contented with a mere superficial

knowledge of the subject, as long as they could obtain results. They wanted to be able to find their latitude or longitude by a mere rule of thumb, without comprehending the principles on which they worked. Others were not satisfied unless they grasped the whole principles of the problem, and these latter, as might naturally be expected, were the most sound and reliable in the work they undertook.

CHAPTER VI.

Former Tests—Modern Instructors.

In former times, the means of obtaining admission to the Royal Military Academy as a cadet were as follows. The Master-General of the Ordnance gave what was called a nomination to a boy. This nomination permitting the boy to attend with others at an examination, by which his fitness for entrance to the Academy was tested. These nominations were the results of interest, or of services on the part of the father, or other near relative of the aspirant to military glory. The boy was examined at the Academy, and either passed, or was "spun," as it was termed, that is, rejected as unqualified. If the boy was rejected, he had no second trial, and the door of the Academy was closed against him.

If there happened to be twenty vacancies at the Academy, probably thirty candidates were allowed to compete, and it was known from past experience how many usually failed, so that it rarely happened that a boy who was qualified, failed to gain admission.

The age at which boys could go up for examination was between fourteen and sixteen, and afterwards from fifteen to seventeen. At the age of seventeen boys had to pass in Latin, French, German, history, and geography, sketching, three books of Euclid, arithmetic, and algebra as far as cubic equations. At a first examination a boy is usually nervous, and does not consequently do his best. After he has had one or two trials, he becomes more accustomed to keep his head, and works calmly and systematically. Under all the circumstances, I consider that the age at which candidates formerly competed, and that only one trial was allowed, the test was as severe as it is now, when several trials are permitted, and when the age of the candidates is so much greater. The mathematical course is much longer for entrance now than formerly; but not greater, considering the relative ages, for a boy of sixteen to be well acquainted with the three first books of Euclid, and as far in algebra as cubic equations, indicates quite as much power of gaining knowledge as for a youth of nineteen to know five books of Euclid, and as far as the differential calculus in mathematics.

It having been considered that the system of nomination to cadetships was too exclusive, open competition was substituted. By this change any youth could come up for examination, and at first there was no limit to the number of trials he might have, so that just to try his powers, and discover how he succeeded, and also to get

accustomed to the form of the examination, he might make his first trial at sixteen years of age, and continue twice a year trying until he gained admission to the Academy, or passed the age at which trials could no longer be made.

The age of the cadets was greater by two or three years than it was forty years ago. Formerly a cadet of nineteen years of age was rare, and nearly every cadet gained his commission before he was twenty. During the time that I was Professor, there were several cadets above twenty-two years of age.

The additional preparation given at the preparatory schools enabled the Instructors at the Academy to deal with subjects of a more professional character than formerly. Instead of such subjects as history and geography being taught to cadets, other and more practical instruction was given. The science of artillery having made great advances, necessitated a long course of study in this subject, instead of, as formerly, only a few notes given to cadets in the practical class. The course of surveying, military reconnaissance, and practical astronomy was considerably extended. Chemistry was taught practically in a laboratory, instead of, as formerly, by a few lectures only, and the whole course of instruction was improved and enlarged.

The system of fagging, which usually degenerated into brutal bullying, and was considered an institution in former times, was weeded out, or reduced to a minimum, so that the last-joined cadet could study without

obstruction. The cadets, as a body, were more manly than in former times, and less given to puerile views; whilst idleness was a very rare thing, the competition for seniority being a sufficient incentive to make each cadet work as hard as he could. The Professors or Instructors, instead of being old gentlemen who, to a great extent, had lost their energy, and were disposed to take things quietly, were young men, and in most cases, were not only thoroughly acquainted with their work; but able to teach what they knew, and after a time gained such a knowledge of the genus cadet, that they found no difficulty in dealing with them.

CHAPTER VII.

Danger of Routine—Crammers—Knowledge *versus* Reason—Languages—Selection of Subjects—Thoughts put into Words—Sir Lyon Playfair—Modern *versus* Dead Languages.

THE experience gained by a Professor during several years at the Royal Military Academy was of a singular character. Each half-year he would have under his instruction from thirty to forty cadets, a new class coming to him each half-year. These cadets were the successful men from various schools and colleges, who had gained admission to the Academy at the competitive examinations, and had, in my case, been two years at the Academy, and then came under my instruction. Several of these had already gained honours in the junior classes, and had given proof of intellectual capacity. Their brain power was in good training, and there was very little trouble necessary to impart information to them. The system of examination prevents the cadet from placing any value on mere superficial knowledge; that which he learns, to be of any use to him, must be learned thoroughly, and he knows this; consequently his endeavour is to avoid going through the motions only of learning, and to get his knowledge

thoroughly absorbed into his brain. Although examinations have great defects and weak points, yet they are probably the best means of testing the amount of knowledge on any subject, which an individual may possess; but to do this, the questions framed by an examiner must be judiciously selected. Some examiners very soon get into a sort of routine system, their questions being not so much to find out what the pupil knows, as what he does not know. There may be certain catch questions very few individuals could answer, unless they had been specially prepared for such questions; and the answering correctly such questions, may be no proof of the extent of knowledge possessed by these pupils. As an example of such a case I give the following, which occurred in my own experience.

Formerly the cadets were examined each half-year by the Professor who had instructed the class, and this Professor assigned the marks that he considered each cadet entitled to for his answers. Considering that it might be possible that an examination by an independent examiner, might aid to give each cadet a better test, outsiders, who had never seen the cadets before, were employed to give a second examination on the same subjects. The marks gained at the two examinations were then added together, and the cadet's total marks gave him his position in the examination. In the majority of cases there was very little difference in the results of the two examinations; the best man in the one was the best man in the other, and *vice versâ*.

Although the examination by the independent examiner was searching, yet out of the eighty cadets per year who passed through my class, not six of these failed to qualify at their examinations during a period of upwards of ten years. Examinations sometimes take the position of a sort of duel between the examiner and the Instructor, and the example I refer to will explain this. During one half-year the class with which I dealt was much above the average, both in ability, and in practical knowledge. The paper, however, given by the independent examiner was what may be called crotchety, and did not give so good an opportunity of testing what the class really knew, as it did of hitting the few blots which they had left. The consequence was, that the report of the independent examiner stated that the knowledge of the class was far below the average, and I consequently was requested to give some reason for this falling off. During the next half-year the class that joined me was inferior in ability, and in application to that which had preceded it, and so much so that I anticipated several failures, and a bad report from the independent examiner. On looking over the past examination questions of the examiner, I suspected that at the coming examination certain questions might be asked. A few days before the examination, I gave a lecture to the class, and by aid of the black-board and a piece of chalk, I explained to the cadets various instrumental and other problems, and made them take these down in their note-books. When the independent

examiner's paper was put before the cadets, I found that eight out of his twelve questions were word, for word, what I had explained to the cadets a few days before. These questions were, of course, all answered correctly, and the examiner in his report stated, that it was the best class he had examined during some years. In reality it was the worst, but I had crammed them for the examination, and had anticipated the questions that would be given.

There are a class of men termed "crammers," whose special qualification is to prepare young men for competitive examinations. It is their business to find out what style of questions are likely to be put at the coming examinations, and great acuteness is shown by some of these crammers in anticipating the actual questions, and causing their pupils to be prepared for these. An outside crammer who did not use this precaution, might not succeed in getting his pupil passed, although this pupil might have gained a more complete general knowledge of the subject than the pupil who passed. As an extreme case of this system of cramming the following will serve as an illustration, it occurred during the time I was Professor. A cadet in one of the senior classes was found unable to draw with even moderate skill, upon being asked how he had managed to pass in free-hand drawing at his entrance examination, the cadet replied that he had not only passed but had passed first in drawing. Surprised at such

a result, the Professor asked the cadet how such a result was obtained. The cadet then stated that his crammer had noticed that a tea-cup, a walnut, and a cockle-shell were the three things given by the examiner for the candidate to draw from nature, and these three models were changed at the various examinations. The examination at which this youth was to prove his skill as an artist, was one at which cockles were due; so during a few days previous to the examination, he drew nothing but cockles, and, as the cadet remarked, "I am good at cockles, but cannot draw anything else;" but he had passed first.

To frame an examination paper, which will test not only the acquired knowledge of a cadet but his reasoning power, requires extensive experience and also considerable talent on the part of an examiner. Some examiners are excessively fond of catches; but these catches are very like riddles, and they are not likely to be answered correctly by those who have never heard them before, but present no difficulty nor call for any display of intelligence, on the part of those who have been crammed with the answer.

Common sense and practical questions to test the knowledge, and the reasoning powers of the examined are required. There will always be a marked difference between answers to these questions; and, instead of giving a few difficult or catch questions, I prefer giving many questions as tests, these questions being so numerous, that they cannot all be answered in

the time allowed for examination, unless a thorough knowledge of the subject has been gained by the youth under examination. I would undertake to frame an examination paper in Practical Astronomy, containing at least a dozen questions, which paper would cause at least ninety per cent. of the Fellows of the Royal Astronomical Society to be "spun" at an examination. These questions would not be mere catches only, but would treat of subjects which I am aware are not generally known to astronomers.

People who have been long accustomed to examine, unavoidably as it were, get into a routine system, they also sometimes look upon examinations as the end, instead of as the means, of testing certain values. What an examination is required to test is, what knowledge of a subject has a pupil gained, and what is his intellectual capacity for applying this knowledge. Some individuals may have thoroughly crammed a subject, and can answer any questions where memory alone is called upon to act. When, however, they are requested to use their knowledge in surmounting a novel difficulty all their cramming is valueless.

In education there is a vast difference between storing the mind with stock knowledge, and strengthening it in order to fit it to reason on novel problems. The mind that is merely stored will get on well enough, as long as it has to deal only with questions which it has previously dealt with. When something out

of the beaten track is encountered, where the stock knowledge does not apply, and where reasoning powers alone can be brought to bear, the stored mind is not unusually more feeble than one which has been left unstored, but has been accustomed to reason. See, for example, the reply given me by a profound mathematician, who assured me that a very weighty telegraph cable would be less affected by the pitching of a ship than one which was very light.

These facts lead one to reason on the various values of subjects, as tests of the brain power of an individual, when given at an examination. All subjects which have no reason to guide their laws, and which are dependent on memory alone, can be acquired in the same manner as a parrot learns to speak. The mind may be stored with a multitude of these subjects, and yet may be very weak as a reasoning machine. Memory alone is required to be able to learn such subjects as history, geography, and languages. There is no reason why one king should have a son with a name such as Richard, and that this Richard succeeded his father at a given date; there is no reason why a certain island situated in a given latitude, should have a certain name; no reasonable explanation can be given why in French there are only two genders for everything, whilst in German there are masculine, feminine, and neuter; nor can we give a reason why the sun is masculine

in French but feminine in German. All these items are arbitrary and without reason, and to acquire a knowledge of them calls for an exercise of memory only not of reason. A child of eight or nine years old will usually learn a language quicker than will a man of five-and-twenty, though the power for reasoning in the latter is far greater. When we come to questions which appertain to geometry and some forms of mathematics, we have to deal with subjects requiring reason. Memory is of use only when we have previously had to deal with a similar question or problem. The intellectual power required for the solution of the geometrical question is not even called into action in the former case. To claim, then, that a knowledge of languages is a proof of brain power, or to make an examination in languages a test of the intellectual capacity of an individual, is, I am convinced, a mistake. Where reason is required it is impossible to cram in a parrot-like manner; where memory only is necessary, it is possible to cram. I would defy any person to "cram" so as to become a first-class whist player. He may learn the leads, what to play second or third in hand, but the combinations are so endless, that all the cramming in the world will not enable him to do away with reasoning. Now, a man may cram chess; he may learn the various openings from books, and all the combinations up to the fourteenth or fifteenth move, and if his adversary has not crammed equally, success will be gained by the player whose memory is the better of the two.

There is a very large amount of what may be termed vested interest in connection with teaching Greek and Latin. A classical education was at one time supposed to be so essential, that no gentleman could be considered fit for society unless he had received it. The practical value of modern languages, and the mental training obtained by the study of mathematics and geometry, were found to be of so much greater value, that classics for a time retired into the background. A very clever ruse was then practised by those who obtained a living by teaching classics; this was to assert that, to *prevent cramming*, classics ought to take a more prominent position, and should at least be put on a par with mathematics. The Educational Department, most anxious to do what was right, and desirous to prevent cramming, assented to this proposition, and the marks for classics were greatly increased. If now it were asserted that to prevent cramming, an equal number of marks ought to be given for rehearsing verses, that were given for solving problems, the absurdity of the proposition would be at once manifested. For classics, modern languages, history, or verses, can only be acquired by cramming, there being no reason to guide one in any portion of these, whereas in the solution of problems, reason alone is the faculty brought into action. If it be required to test the powers of memory of a pupil sent for examination, then classics may serve the purpose; but the acquisition of such classical knowledge is not surely of such practical use to an officer, as a

knowledge of modern languages, which knowledge would quite as efficiently test the pupil's powers of memory.

When selecting subjects which are to be taught to a pupil, these subjects should be for one or other purpose. They should be of practical value as acquired knowledge, to be stored in the memory, and made use of in after life, when a practical application of them can be made; or they should be selected on account of their efficiency for strengthening the reasoning powers of the brain. In many instances, the practical use of a subject may be combined with its strengthening character. In other cases, only one of these advantages may be possessed. When we have a choice of subjects to be taught, those should be selected which possess these qualifications in the most marked manner. If we required a subject which would call into action, and strengthen the brain by mental exercise, there would probably be nothing better than solving numbers of three-move chess problems. A great amount of mental power is required in order to solve such problems. Patience is also requisite, but we never find in any school that a day is devoted to such a study—and why? It is because the power to solve chess problems is never likely to be of much practical value in the every-day duties of a man's life. There is no denying the fact that such a study is an excellent training for the mind; but it is not taught in our schools, because, without probably reasoning deeply on the

reason why it is not taught, schoolmasters feel that it might be considered a waste of time. Let us now consider the value of the very long course of study which even now is carried out in connection with Greek and Latin; no one, other than a devotee of the classics, could ever assert that the study of the dead languages called for any mental exercise appertaining to reason. A very small amount of reason supporting a good memory is all that is necessary in order that a man shall acquire a knowledge of a language. Then we next inquire what is the practical use in after life of a great knowledge of Greek and Latin. There are a few works which such knowledge may enable the student to read in the original; but it is questionable whether three or four years of study receive an adequate return, when this comes in the form of an ability to read a Greek or Latin work, or to be able to translate some quotation given in the dead languages. It has been asserted that no man can understand his own language well, who has not learnt Latin thoroughly. It is possible that such a man may not be able to trace each word to its original root, but to do so is merely a matter of minor importance, and is, I consider, less likely to be obtained by a knowledge of Latin than it is by a knowledge of Sanskrit or Persian. What, after all, is the object of language? One cynic has stated that it is to enable us to conceal our thoughts; but the usual object of language is to enable us to make known our thoughts. A thought or an idea

is immaterial, a sort of spirit form invisible to the eye of another man, and inaudible to his ear. When this thought is materialised, either in spoken or in written words, it is manifested to our fellow men. If the thought which may be high, pure, and true, be materialised imperfectly in words, it fails to impress on the mind of the hearer, or reader, the identical idea which emanated from the thinker's mind; it would be like placing a great spiritual nature in the material body of a deformed man. Take, for example, some of the ideas which have emanated from our poets, and note how these ideas have been expressed in words. We all know that if a man does not use his arms or legs during many months, they become stiff and useless when first called into action, and the brain also requires constant exercise to keep it healthy and strong. Here are words which express an idea, but they are awkward and imperfect. A poet, however, has expressed this same idea as follows:

> There is a firefly in the Southern clime
> Which shineth only when upon the wing.
> So is it with the mind; when once we rest,
> We darken.

As we read these words we realise completely the idea which was in the poet's mind. He has so materialised his thought, that we for a time feel with him and think with him. The choice of the words has enabled him to convey to others the thoughts emanating from his brain, and to do so is undoubtedly the object of language.

In the present day men, especially soldiers and sailors, are great travellers, and they find it not only pleasant, but often essential, to express their thoughts or wishes in words. Of two men who were travellers, who would be able the better to express his thoughts as he travelled over the world, the classical scholar who was learned in Greek or Latin, or the man who could speak French, German, or Spanish? Until one universal language is agreed upon for the whole world, and when consequently no man need, except for amusement, learn more than the universal language, beside his own, men gifted with much learning, and full of ideas, are practically dumb when they are in a country of the language of which they are entirely ignorant. Thus, for the soldier, the sailor, or the traveller, I consider that the years of study devoted to the classics is so much waste time; and when I have seen the number of marks assigned to Greek and Latin at the entrance examinations for Woolwich, I have regretted that those officials who consented to this arrangement, on the imaginary and untrue principle that it did away with cramming, had not been more clear-sighted, for the real fact is, that languages can be crammed far more easily than can mathematics or geometry.

"Sir Lyon Playfair's complaint in his British Association address of the neglect of science in the public schools is perfectly justified by the facts. The truth is, that in spite of all the talk of late years about extending the curriculum, the education of the mass

of boys at the public schools is still almost exclusively classical, the so-called 'modern side' being in most cases merely a device for disposing of the 'classical asses.' That this is so is proved by the results which have lately been published of the Midsummer examination for the Oxford and Cambridge certificates. The number of boys who passed in Latin and Greek was 703 and 673 respectively, whilst science in all its branches was credited with only 131. The neglect of modern languages is hardly less remarkable, and perhaps more reprehensible. There were 263 successful candidates in French, 94 in German, and 113 in English. The head masters are, no doubt, primarily to blame in the matter; but the British parent is an accomplice, for he still clings to the notion that the only proper 'education for a gentleman's son' is such as will be more or less useless."—*British Association*, 1885.

I have remarked in many instances that a last joined cadet stood high in his class when he first joined the Academy; probably he was among the first six in his term. As he advanced from examination to examination, he gradually descended until he was finally among the last dozen. When under my instruction, I have found such a cadet very slow at grasping an idea, and deficient also in the capacity for reasoning out a problem, which was not bound by some distinct rule. On looking over the marks gained by such cadet at the entrance examination, I almost invariably found that he was a good classical scholar, and had gained a high position at entrance in

consequence of his knowledge of Greek and Latin. This stock knowledge, although it gave him a good start at entrance, and an accumulation of marks to begin with, was yet of no further value, as examinations in Greek and Latin were not continued after entrance to Woolwich. The mind of this cadet had been merely stored, not strengthened, so that when he had to encounter problems requiring reasoning powers for their solution, he was at a great disadvantage. To him such a novel exercise of the brain was as difficult as for a cavalry soldier, who almost lived in the saddle, to ascend the rigging of a rolling ship.

It might be interesting to know what advantage, during their subsequent service in the army, had been derived by these classical scholars from their knowledge of Greek and Latin. I do not refer to advantages which they may have obtained in private matters, outside of their military duties, but to those connected with their profession. Have they at any time, or under any conditions, derived sufficient return for the three or four years of study, which they must have expended, in order to take rank as classical scholars?

In too many instances the old fable of the fox who had lost his tail is practised in this matter. Men who have been made to occupy four or five years of their lives in studying classics seem anxious to impress on others the absolute necessity of a similar course, in order that a man should be considered educated. The "nothing like leather" idea is thus beautifully exempli-

fied. If I have taken a double first for classics, and am then told that this result is no proof of great intellectual capacity or of reasoning power, my performance is stated to be no great thing after all. I know what a very long time, and what an exertion of memory, was required to enable me to reach such a pinnacle, and it would be gratifying to me to see others follow in my footsteps, and fail to reach the same altitude. Also, if my bread was to a great extent dependent on teaching classics, I certainly should be disinclined to underrate the value of that knowledge which I possessed; and if, by any endeavours of my own, or of my friends, I could induce the educational authorities, to insist that no person should be appointed to any Government appointment unless he were a very good classical scholar, there would at once be placed on my acquired knowledge a value, not according to its real worth, but perhaps equivalent to the number of years that I had wasted in its acquisition.

But the question is not one of opinion merely, but of actual fact. Is there anything requisite in the learning of a language that requires or calls forth great, or even moderate, reasoning powers? Is it not memory, application, and ear for sound, that enables a man to learn a language? What, may we ask, is there in the dead languages which causes them to be considered superior as intellectual studies to the modern languages? If any such peculiarities exist, surely they have not been clearly put before the public. If, then, no

such real value exist, the acquisition of classics for a soldier or practical man, is a mere waste of time. In past ages the knowledge of Latin was of some use, because the educated men of nearly every country learnt Latin, consequently it became a sort of universal language in which men of different countries could clothe their thoughts. Such, however, is not the case at present; and although the use of this language has practically died out, yet its admirers seem to consider it still necessary as a study, and are actually acting like the bellows-blower of an organ, who continues blowing there long after the organist has left the instrument.

As a boy I commenced by studying Greek and Latin at a classical school, and devoted two or three years to these subjects; I then was put to French and German. Except for reading a few books in the original I have never found any practical use for my classics, but for my French and German I have found almost daily use. A knowledge of German enabled me, after a few days, to converse with the Dutch Boers, there being great similarity in the two languages; and when travelling in Europe and Canada, French was essential.

In spite, therefore, of all the nonsense that has been written and spoken about a study of classics preventing cramming, and being a grand exercise for the mind, it is a fact that any language, no

matter whether ancient or modern, requires, not reason, but memory, ear, and application, and each language ought to be esteemed on account of its practical value as a means by which we can convey to the greatest number of our fellow-creatures our thoughts, wishes, or wants.

CHAPTER VIII.

Value of Mathematics—Course of Mathematics—Mere Mathematical Minds—Edgar Poe's Remarks—Value of Geometry.

MATHEMATICS always have taken the first place at examinations at Woolwich, and although at one time classics obtained nearly as much reward as mathematics, yet the dead languages never stood first. It has been almost universally agreed that the study of mathematics calls into action not only memory, but reason. This science creeps, as it were, over the ground like the movements of a serpent, leaving no portion untouched. Given the preliminary data in an accurate form, the routine of mathematics will, when accurately followed out, give us an answer which we can depend upon. A mathematician can test certain statements, and can assert that these are either impossible or may be true; he can also, by the rules which he knows, solve problems which to the mind untrained in mathematics appear either insoluble, or very complicated. In matters of every-day life, the mathematician brings his science to bear, he notes where unnecessary

force or labour have been employed by ignorant people, and he studies economy in labour. To solve many of the problems which he encounters, he has to reason and consider, because mere routine will not supply him with the means by which he can obtain solutions. As a comparative test of the power of brain possessed by an individual, there can be no doubt that an examination in mathematics is far superior to one in classics. Supposing two individuals have been equally trained by an Instructor, the results of an examination in mathematics would give a far more accurate value of their relative brain power than under similar conditions an examination in classics. The certainty with which a mathematician can state that an assertion must be false, or may be true, gives him a great advantage when dealing with ever recurring problems. His special training leads to a love of accuracy in details, and his mental machinery is thereby so trained, that it works smoothly and accurately, instead of in an erratic manner. A mathematical or geometrical truth there is no denying; and all the argument in the world will not cause a mathematician to doubt for a moment that the three angles of a plane triangle amount to 180° exactly, that two sides of a triangle are greater than the third side, that if X^2 amounts to 16 then X must be equal to 4. If a man were to inform a mathematician that he could lift a bar of iron of uniform thickness more easily

by raising it from one end than from the middle, he would know that this man was under a delusion, because he would know that to raise this bar from that point where the centre of gravity was situated, was the point where the least lifting power was required. A mind trained to mathematics takes a pleasure in the solution of problems; to such an one it is not labour, but a pleasant exercise. It is similar to the pleasure which a healthy and active man takes in a ten-mile walk in the country, which walk, to the individual who rarely used his legs would be a fatigue and a labour almost beyond his powers. Mathematics not only strengthen the mind, but also store it with stock knowledge. This science, therefore, has deservedly maintained a first place, as a test of the mental capacity of our candidates at competitive examinations. It is possible to cram mathematics; but such a proceeding is more difficult where mathematics are concerned than it is where classics are dealt with. An examiner who selects his questions judiciously can easily form a series of questions, the solution of which cannot be obtained by cramming only. If an examiner get into a system of routine, an observant crammer will soon discover this, and will train his pupil for a special examination. This pupil may thus gain a high position at the examination, but may know very little of the whole subject of which the special paper is supposed

to be a test. In fact we too often have the cockle-shell example repeated.

The course in mathematics which I consider the most useful both for storing and strengthening the mind is a thorough knowledge of single and double rule of three, fractions, decimals, square and cube root, simple and quadratic equations, plane and spherical trigonometry, conic sections, statics and dynamics, roofs and arches, and projectiles. A thorough knowledge of these subjects, and their practical application, gives the student enormous power, and cultivates his reasoning faculties. He has also a large amount of stock knowledge, which he rarely, if ever forgets; and if he has not been over-crammed, but has gained his information quietly, so as not to produce mental indigestion, he will, when free to do what he chooses, continue to find a pleasure in increasing and practising his knowledge.

Whilst there are these advantages in the study of mathematics, there are certain drawbacks which must be guarded against. During my experience at Woolwich upwards of twelve hundred cadets passed under my instruction and observation, besides more than one hundred officers belonging to various branches of the service, and the conclusion I arrived at was, that the most stupid man is he who is a mathematician only. In order to derive all the benefit which a knowledge of mathematics gives to its possessor, there must be great

powers of observation and an active imagination. Without such aid, mathematics descend to so complete a system of routine, that common-sense is ignored, and when a case occurs where common-sense and routine clash, common-sense is ignored, and routine triumphs. I cannot give a better example of this system of intellectual imbecility than one which occurred to me when in India. In order to draw our pay it was necessary to forward, on the last day of each month, a certificate stating that the individual was then alive. During the months of June and July I was travelling in the Himalaya, in wild districts where no Englishman was within miles. On my return to Simla I forwarded a certificate on the 31st August, stating that I was alive, and requesting that my pay for June, July, and August should be paid to my bankers. In reply I received a communication stating that my pay for August would be credited to me, but that there was no certificate to prove that I was alive in June and July. I stated that as I was alone in the mountains during those two months, except that some hill-men were with me, who could neither read nor write, I could not supply such certificates, but that I thought it could not admit of question that if I was alive in August I must have been alive during the June and July previously. I was informed, however, that my pay for those two months could not be granted unless the required certificates were forwarded. I then suggested that I would draw my pay for a quarter ending the 31st August, and that,

as I was alive at the end of the quarter, the conditions for payment were similar to drawing my pay at the end of each month. I pointed out, also, that if this argument or objection were sound, there was a defect in the present system of payment; for although a certificate might prove that I was alive on the 30th June, there was no evidence to prove I might not have been dead from the 1st to the 29th June. It required several additional letters, and, finally, a special order, to enable me to draw my pay during June and July. Here was a case where common-sense was utterly ignored, and a sort of mathematical routine was adhered to.

On one occasion when instructing cadets in a method of finding heights and distances, by using the sextant and a scale of tangents, I directed a cadet who was a mathematician to find the height of the Rotunda at Woolwich. Instead of being contented to employ the simple method I had explained to him, this cadet was anxious to obtain minute accuracy by measuring various angles and working out the whole question trigonometrically. After occupying at least ten times as many minutes by his method as he would have occupied by the more simple plan, he announced that the height of the Rotunda was $627\frac{32}{100}$ feet. I intimated to him that St. Paul's was not as high as this, and that he must have made a mistake. The cadet re-examined his work, and asserted that it must be correct, as the principle he had adopted was minutely accurate. Suspecting what the error was, I looked over his work, and found

that in the final answer he had put the decimal point in the wrong place; this being the only error, the true height by his process being $62\frac{7}{10}$ feet instead of 627 feet.

Such ridiculous proceedings are due to the mind having become so over-cultivated as regards routine, that common-sense and observation are dead; an individual cannot perceive what is true or what false, unless he can prove this by his routine mathematics. His imagination, and even his perceptive powers, are considered dangerous and likely to delude, and recorded facts made known to us by our perceptive faculties, are not considered valuable, or even true, unless they can be demonstrated to be so by a mathematical formula. Another defect of the merely mathematical mind is that the preliminary data are often slurred over, and are incorrect. All that is built on this is no doubt true, but the first foundation is not true, consequently all that is built on it is false.

Edgar Poe, whose writings exhibit great powers of mind, has ably expressed somewhat similar views. In his "Purloined Letter" he says, in reply to the statement that the minister was a mathematician, but no poet:

"'You are mistaken. I know him well; he is both. As poet and mathematician, he would reason well; as mere mathematician, he could not have reasoned at all.'

"'You surprise me,' I said, 'by these opinions, which have been contradicted by the voice of the world.

You do not mean to set at naught the well-digested idea of centuries? The mathematical reason has long been regarded as *the* reason *par excellence.*'

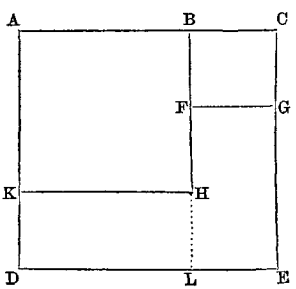
"'*Il y a à parier,*' replied Dupin, quoting from Chamfort, '*que toute idée publique, toute convention reçue, est une sottise, car elle a convenue au plus grand nombre.* The mathematicians, I grant you, have done their best to promulgate the popular error to which you allude, and which is none the less an error for its promulgation as truth. With an art worthy a better cause, for example, they have insinuated the term analysis into application to algebra. The French are the originators of this particular deception; but if a term is of any importance, if words derive any value from applicability, then analysis conveys algebra about as much as in Latin *ambitus* implies ambition, *religio* religion, or *homines honesti* a set of honourable men. I mean to say,' continued Dupin, 'that if the minister had been no more than a mathematician, the prefect would have been under no necessity of giving me this check. I knew him, however, as both mathematician and poet, and my measures were adapted to his capacity.'"

Geometry differs from mathematics mainly, because in geometry, diagrams take the place to a great extent of mere ciphers and signs; we see a geometrical solution much more easily, the figure is before us, and we are on firmer ground. A clerical error in geometry is almost impossible, whereas in mathematics it is not only possible but probable, and I have remarked that some

of the best mathematicians fail to add up correctly a long row of figures.

As an example of the difference between a geometrical and a mathematical proof I will give the well known problem of Euclid, that: If a straight line be divided into any two parts the square of the whole line is equal to the square of the two parts together with twice the rectangle contained by the parts.

First we will prove this geometrically, and take AC, as the line divided at B, the square of the whole line is represented by the square ACED. The square of the two parts is represented by the two squares AKHB and BFGC. The length AD is equal to AC. If we take BC from AC, KH is equal to AB, and AK is equal to KH, therefore KD is equal to BC, and the rectangle HKD is the rectangle formed by AB and BC. Also, FG is equal to BC, and FB is equal to BC, and CB is equal to CG. Therefore, GE is equal to AB, consequently the rectangle FGLE is formed by two sides equal to AB and BC. We have then two rectangles formed by AB and BC, and two squares, viz., AB squared and BC squared making up the whole square on AC. It scarcely requires a demonstration to prove this problem; it can be seen at a glance. The mind grasps it and comprehends it.



Let us now deal with the same case mathematically. Let $x + y$ be the two parts of the line, then $(x + y)^2 = x^2 + y^2 + 2\,x\,y$, then multiply $x + y$ by $x + y$ and thus obtain the square, and we have $x^2 + 2\,x\,y + y^2$, the same thing; but I do not think this proof is as easily comprehended as is the former. There are some problems which can be solved by geometry only, mathematics not being able to touch these. Take for example the problem that in a right-angled triangle the square of the hypothenuse is equal to the square of the other two sides. Take also another and more important problem, viz., that the earth is not a flat surface but must be spherical, mathematics cannot prove that the earth must be spherical, but the science of geometry can prove it (the proof will be found in Appendix A). Geometry has a power in many cases beyond mathematics, and in many cases is more explicit and better comprehended. When, then, I hear men sneering at geometry and asserting that it has no power, and as a science is inferior to some theoretical speculations of their own, I feel certain that such men have no claims to be considered either scientific or sound, but are usually quacks who, having gained a temporary reputation by starting on false premises some popular theory, are afraid that the unsoundness of their theories will be disclosed by geometry, and, therefore, anticipate and endeavour to underrate such a result, by casting doubt on the testimony or accuracy

of the witness who will appear against them. From a very long experience in instruction I am convinced that a study of geometry is the very best that can be given, in order to strengthen and store the mind, and give it a clear insight into any problem. The imagination is called into action by geometry, and the detail proofs are the soundest that can be given.

CHAPTER IX.

Storing the Mind—Value of Various Sciences—Course at the Academy —Military History—Disasters in South Africa—Fighting the Zulus —Selection of Subjects—New Sciences—Examinations.

It will, I believe, be evident from what has been written in the preceding chapters that there is a most important difference in the effects on the mental training of individuals of various subjects of study. Some subjects store the mind only with a collection of facts, and memory (which some of the lower animals possess in greater strength than does man) is alone called into activity. Other subjects are not to be mastered by memory alone; reason must be used, and judgment employed to solve problems connected with mathematics and geometry. The study of languages does not, I maintain, call for great intellectual exertion, because languages are formed on arbitrary laws. Any man who can learn quickly a piece of poetry could acquire a language rapidly. During six months I learned to speak the Zulu language sufficiently well to be able to converse easily with the natives, and the mental exertion used to obtain this result, was of the same class as was called into action to learn a piece of poetry. From

having previously studied mathematics, geometry, and practical astronomy, I could feel that the trial to my mind whilst learning this language was slight, as regards reasoning faculties, to what it had been when studying either of the former. A double-dummy problem at whist, or a mate in a three-move problem at chess, required the expenditure of far more brain power, than was ever employed in acquiring a knowledge of the Zulu language.

There is, perhaps, no science which is more useful practically than a knowledge of chemistry, and there is scarcely any which is more overrated as regards the mental power required for its acquisition. Chemistry is a purely experimental science. There are a number of facts which have been ascertained. It has been proved by repeated experiments, that certain results follow certain conditions, and these results are invariable when the conditions are fulfilled. The accumulation of experimental facts forms the stock knowledge of the chemist. The lecturer on chemistry, who astonishes and amuses his audience by a series of successful experiments, demonstrating that certain brilliant results follow certain combinations, is usually regarded as a man of great intellect, and a philosopher whose opinion on other subjects, which he may have examined only superficially, ought to be accepted *ex cathedrâ*. There is no display for great reasoning powers in chemistry, as it is purely an experimental science; and I would undertake that I would train a man almost devoid of reasoning powers

to become an excellent chemist, if I trained him in a laboratory during some five or six years. The value of a training in chemistry is the stock knowledge gained. It is not that the mind is strengthened as a reasoning machine by such a course of study, and thereby fitted to judge correctly on other subjects, but it is useful on account of the store of facts recorded in the memory. I think that one of the most incompetent reasoners I ever met, was a great chemist, and he was also one of the most self-opinionated men. One great advantage of a study of chemistry is that it cultivates the powers of observation. The chemist will, if possessing fair intelligence, be more observant than men who have not studied this, or some other similar science. The foundation of this, as of many other sciences, is dependent on observation; that which tends to cultivate the powers of observation tends to strengthen the capacity of an individual. When we perceive our giant steam engines at work, and note the grand results of this agent, we wonder at the absence of observation of the men of former ages, who failed to observe the power of steam, even though so feebly manifested as it was in raising the lid of a kettle in which water was boiling. Any science, or study, which not only stores the mind, but cultivates the powers of observation, is valuable; but we must not mistake either an accumulation of facts in the memory, or quickness of observation, for a capacity to reason. The two powers are entirely different.

When we teach, it does not necessarily follow that we educate. We teach a child its letters, and we have by so doing cultivated its powers of observation, but we have not by this means called its reasoning faculties into action. We have given to this child a certain knowledge, which, being extended, will enable the child eventually to read and write. If our alphabet consisted of ten thousand letters instead of twenty-six, and a child learnt the whole of these, it would merely have resulted in storing the child's mind, and the importance of having gained such knowledge would be estimated by the value of the stock information contained in each letter. This is very much the case with chemistry and other similar subjects, such for example as languages.

At the Academy cadets are taught military sketching and surveying, and also military reconnaissance, a knowledge of geometry is requisite in order that surveying can be understood, and as the form of the country differs in various places, the surveyor has to reason as to the best manner of making a survey, and where to measure distances, and to take angles. When, in addition to a sketch of the country, a reconnaissance is required, great powers of observation are necessary. Those features of the country which would affect in any way military operations, must be noted and described. The observation requisite to make a good reconnaissance is the same that would be required when an officer has command of troops, and has to

carry on operations in a strange country. I place, therefore, great value on the mental training derived from a study of military reconnaissance, as it calls into action those identical faculties which are required by an officer on active service.

When I joined the Academy as Instructor I found that the cadets were kept nearly two years in the junior classes learning nothing of contouring, sketching, or surveying, but were occupied in making representations of hills by what is termed hachuring, that is, scratching with a pen in such a manner as to represent hills. Some cadets could never be made artists; and even those who were successful would, if they neglected this work during a few months, lose the skill which they had gained, and it would require two or three days' practice, before they could regain their neatness in drawing.

Being desirous of making an alteration in this system, and substituting a more useful course of study in this department, I complained to the Governor that cadets came to me utterly unacquainted with the system of representing ground by contours. They were also ignorant of either the compass or sextant, and I suggested that these subjects should be taught to cadets six months before they joined the practical class. It was a long time before I was able to obtain this innovation, but at length succeeded in doing so, and the use of the compass and sextant, and military sketching, were taught in the class which

would on the following term join the practical class. By thus having the elementary portions of military surveying taught six months earlier than formerly, I was enabled to give the senior class a longer course of instruction, so that each cadet before leaving the Academy could not only make a military contoured sketch, but could find his latitude and longitude by sextant observations, and was practically acquainted with several methods of range finding, and had a fair grounding in accurate surveying. I considered that the additional instruction which I was thus enabled to give, was more likely to be of use to an officer in after life than a capacity to hachure a drawing like copper-plate, after he had recovered by practice the skill in execution which he had formerly possessed.

Two other important subjects were taught to cadets, viz., fortification and practical artillery; both these subjects are entirely professional, a knowledge of both being absolutely necessary to every officer of artillery and engineers. Both these sciences require reason for the solution of the various problems, and also store the mind with stock knowledge. They deservedly occupy a high place in the course taught at the Academy, the marks for these two subjects being very liberal.

Military history is a subject which was not taught when I was a cadet, but was introduced during the time I was Professor. The information to be gained

from a knowledge of military history is most valuable to an officer, especially if the lecturer select his subjects judiciously. We probably learn more from disasters than we do from success, and a record of the disasters which at various times have occurred to our troops; and an investigation of the causes which have led to these, ought to teach us where our weak points are, and how to guard in future against a repetition of these.

In the year 1858 I published a work, entitled "Sporting Scenes among the Caffres." A few years later I published another, entitled "Tales at the Outspan." Again, a few years later, another work, viz., "Adventures of Hans Sterk" (afterwards renamed "Among the Zulus.") In each of these works I called attention to the courage, cunning, and method of fighting of the Zulus. I stated in these works how the Boers had laagered their waggons, protected them by wait-a-bit bushes, and had beaten off ten times their number of Zulus with scarcely any loss to themselves. Here was actually a military history of these people and their method of fighting. Our troops are sent against these Zulus, and those who have the direction of affairs seem so grossly ignorant of everything connected with the people against whom they are sent, that they neglect the commonest precautions. They are like children ignorant of the qualities of gunpowder and fire, and play with these articles as they would with harmless marbles or dolls. What is the result? A

slaughter at Isandula of over a thousand of our soldiers, a loss of our prestige in the country, and a necessity for additional troops to be sent, and a campaign which cost us hundreds of thousands of pounds. Then, again, who that had known anything of the Zulus would venture far into their country when at war with them, and then off saddle, and sit quietly down in a mealy garden to take lunch? Such a situation was the place of all others where one could be stalked with the greatest ease. Select an open plain, with no bush or other cover within five hundred yards, and even then there would be risk; but to commit such an act of folly as to sit down in a mealy garden, was like an ostrich hiding its head in a bush, and believing that its pursuer could not see it. A prince, on whom probably important interests connected with England rested, was thus massacred, and the affair was considered, if not an accident, as due to an inexperienced officer galloping off, when he ought to have stopped to be assegaid with the prince. This disaster was due to neither assumed cause; it was, in reality, due to the ignorance of those whose duty it was, to see that such an important charge was not placed under the direction of any one who was not well versed in the habits and cunning of the Zulus. Military history, had it included a description of the method always adopted by such people as the Zulus, would have taught the Prince Imperial, and those to whom he was entrusted, that to act as he did was not an act of bravery—it was a proceeding similar to that of a youth who would sit on

a barrel of gunpowder and smoke a pipe, in order to prove how brave he was. The Prince Imperial was under my instruction as a cadet, and I shall have more to say about him at a future page; but I can here state that from what I know of him, I am certain that, although brave and daring, he would never have committed such an act as that which caused his death, had it been explained and shown to him that he was giving an enemy every advantage, and placing himself in a position of helpless risk.

Much is to be learned from military history, especially from a history of our failures. It is unnecessary to refer to another of our disasters in South Africa, viz., that at Majuba Hill. The cause of this was as avoidable as that at Isandula, and was due to a great extent to underrating the enemy.

Now I maintain that had the leading minds in these affairs been trained by a knowledge of facts connected with their enemy, and a capacity for reasoning thereon, neither disaster could possibly have occurred. In two cases it was merely carelessness and a want of proper precaution; and in the other, false reasoning from incorrect data. Bravery alone will never win a battle or avoid a disaster, any more than it will win a game of chess, or avoid a checkmate. That which is necessary in our generals and soldiers having any command, is accurate observation and correct reasoning thereon. There may be many persons alive now who may remember when our Volunteer movement first took place—and it was

pointed out that whilst it was necessary that our men learnt to shoot accurately, it was equally as necessary that they learnt how to seek cover—that a distinguished officer wrote to the papers to say that he hoped our Volunteers would do nothing so cowardly as to sneak behind trees or banks of earth, but that they would stand well out in the open, and show the enemy that they were not frightened. As well might a chess-player recommend that, as soon as possible, you should move your queen up to be taken by a pawn, to prove you were not afraid of your adversary.

A little mental training in such cases would be desirable; and a lecture on the art of war might be of benefit to such types of mind, with the hint that "fools rush in where angels fear to tread."

We have in the preceding cases examples of the exact type to illustrate the mental defects of those who had the management of affairs lately in South Africa. If another campaign were undertaken against the Zulus, the commanders would not incur the risk of another such disaster as that of Isandula, nor of such as occurred to the Prince Imperial. And why? Because such commanders have now the stock knowledge gained by the past events, and, remembering what has occurred, they would guard against a repetition of a similar result. Here we have exactly the type of mind which is stored but not strengthened. Given other conditions varying considerably from those at Isandula, and an equally fatal error would be made by the class of mind which

was only stored. The conditions being novel, there would be nothing to be gained from past experience, and the unreasoning mind would be incompetent to reason on what results would most probably occur. The great difference between the unreasoning and the reasoning mind is, that the former becomes wise after the events have occurred; the latter is wise beforehand, and, seeing what is probable, guards against the occurrence of those events, and often prevents their taking place. The former pays dearly for his experience, the latter perceives what his experiences are likely to be, and prevents by proper precautions their taking place.

Let us now reason on the facts before us. A small army is in the Zulu country. It is known that the Zulus have from 15,000 to 20,000 armed men, that these Zulus are brave as lions and admirably disciplined in their peculiar system of warfare. It is known that an army of these men can travel forty miles a day. It is known that every artifice practised by the hunter is understood by the Zulus. When no Zulu is seen within miles of a camp or fort during several days, then look out, because it is tolerably certain that numbers of these men are concealed near. Surprises, ambushes, and rapid attacks are their special system of drill. Here are the preliminary facts on which to reason. Place an army of some 1,500 men only, in an open country, take no precautions to laager the waggons, to form a defence of any kind against a rush of overwhelming numbers, and what is the solution of this problem? The man whose

mind was merely stored would be uninfluenced by the preliminary data, he would say there is no record of the Zulus ever having rushed down on the bullets and bayonets of regular troops; besides, there is no sign of any Zulus near; we can sit down in our camp without fear, and it would be unnecessary labour to take the precaution of forming a defence camp. The reasoner, however, would at once conclude that the conditions were such that an attack was not only possible, but probable. He would endeavour to identify himself with the enemy, and to imagine himself a Zulu chief with 10,000 daring men under his command. He would see the defenceless condition of the camp, and the means by which it could be best surprised, and he would feel how great a temptation such a state of affairs would be to a Zulu chief. He would, consequently, almost realise the future, and would see in a sort of vision what was likely to occur. He would be like a good whist-player, who, knowing the best trump was against him, and that the adversaries required only one trick to win the game, would perceive that it was unnecessary to proceed further. The bad player would, however, assert that he would play on, as he held two aces which must make, and he would fail to perceive that no matter how many tricks he could make, the best trump must win the game. The reasoner, therefore, would guard against a disaster, although a similar one had never previously occurred; whereas the man whose mind had been only stored would not reason at all, but would, after the disaster,

store this in his memory as a thing to be guarded against.

It may possibly be asked what connection is there between the course of study carried on at the Royal Military Academy, and the disasters that occurred in the Zulu and Boer wars. There is a most intimate connection. The course of study hitherto carried on at the military colleges, including the Staff College, has been mainly to store the minds of individuals, not to enable them to reason. Consequently, when men have been thus trained, they are little superior to the greatest dunce, when they encounter novel conditions, and it is necessary to reason on these. The experience of past ages also indicates that those men whose minds are the most completely stored with the facts, or supposed facts of any science, are almost invariably those who are the least able to reason on any novelty which is brought forward in connection with the science in which they possess extensive stock knowledge.

To select those subjects which will be of the greatest advantage in after life, to each individual, is a matter of the greatest importance. Young men now are, as it were, engaged in an intellectual race. From the ages of twelve, to seventeen or eighteen, youths are preparing for some competitive examination. Their time is limited; they cannot afford to waste it in acquiring a knowledge of subjects which will not pay at an examination. As I before remarked, we invariably find, some time or other, a use for anything that we have learned;

but when we are engaged in a race, we cannot devote time to the study of outside subjects. The whole power of our minds must be occupied in acquiring a knowledge of those subjects, which will enable us to win a good place at the goal towards which we are struggling.

When preparing for my entrance examination at Woolwich, a friend suggested to me that I should study architecture, as it would be very useful to me in case I joined the Royal Engineers. At that time every hour was occupied in studying mathematics, Euclid, French, German, Latin, history, and geography. I told my friend that I was like a man in a sinking ship, who had to swim on shore to save his life, my shore being the Academy; that no doubt two or three thousand sovereigns would be of great use to the man when he reached the shore, but if he loaded himself with these sovereigns, he would sink, and never reach the dry land; so, in my case, I could not afford the time, or the brain power, to learn a subject which was not of practical use at my coming examination.

A variety of subjects have been selected, as those in which candidates are to compete at the entrance examination at the Academy. These subjects, and the marks to be allotted, have been selected by certain officials connected with military education.

It may be reasonably assumed that the subjects on which a candidate is to be examined, have been selected, because a knowledge of such subjects is necessary, or likely to be useful, to an officer in either the Royal

Artillery, or Royal Engineers. This certainly would be the first reason which should influence those who select the various items to be learnt by candidates for admission to Woolwich. To select some science for study, such, for example, as ship-building, which an officer of Artillery would in his future career rarely if ever find of practical utility, would be a great mistake; so that we may perceive what knowledge is supposed to be most useful to an officer, by noting the various items directed to be acquired, and the supposed relative values of these can be estimated by the marks given to them.

If we find that some subjects were put down on the list which do not appear to possess those useful qualities, which are certain to be required by an officer, then this proceeding must be due to the fact that more useful subjects do not afford any test of the mental capacity of those who come up for examination. Or, it is assumed, that a knowledge of such subjects must be gained, before the candidate can study those sciences which will be useful to him.

During the past fifty years or so, various sciences, formerly almost unknown, have been brought into notice, and are being made of practical use. So that young men in the present day have much more to learn than had their grandfathers. Such sciences as those of steam, electricity, geology, and many others are comparative novelties, when compared with mathematics or classics. The science of artillery alone has, and is, making such rapid progress, that if an officer is absent

on leave for a few months, he has to go to school again, in order to learn the improvements, or alterations, which have been introduced during his absence.

The allotment of marks for entrance examination is as follows:—

	MARKS
For Mathematics	4,000
„ English Literature	2,000
„ Latin	2,000
„ Greek	2,000
„ French	2,000
„ German	2,000
„ Any other language such as Italian, Russian, Spanish, or Hindustan	2,000
„ Chemistry, Heat, Electricity, and Magnetism	2,000
„ General and Physical Geography and Geology	2,000
„ Free-hand Drawing	1,000

Of these nine, subjects candidates will not be allowed to take up more than four, exclusive of drawing.

Previous to the candidate coming up for Woolwich he must satisfy the commissioners that he knows fairly the following, viz., in mathematics and arithmetic, the use of common logarithms, and algebra, including equations, progressions, permutations, and combinations, and the binomial theorem; Euclid Books I. to IV. and VI., plane trigonometry, including the solution of triangles. In languages, one language must be known sufficiently well to be able to translate from the language, and to answer grammatical questions.

24443

Writing English in a good legible hand from dictation, and English composition.

The elements of geometrical drawing, including the construction of plane scales, and the use of simple mathematical instruments.

Geography.

We will suppose that a youth finds he has no head for mathematics, beyond that amount required for the preliminary examination, he can then cram those subjects requiring memory alone, viz., Greek and Latin, as one subject, for which 4,000 marks are given; any other language, say French, 2,000 marks; English literature, 2,000 marks; chemistry and heat, 2,000 marks; geography and geology, 2,000 marks. The total number of marks possible to be gained being 12,000. Decimal 7 or 8 might be obtained out of these, making 8,400 or 9,600 marks for subjects depending almost entirely on memory or ear.

To pass successfully in all these subjects gives no proof that the candidate possesses any other mental capacity than a good memory, and the power to cram certain rules. The list given above consists of what is usually erroneously defined as a thoroughly sound English education. If for the word education we substituted teaching, we might be nearer the mark. We have not educated the mind by cramming it with all these subjects, we have merely stocked it with rules, and words. A youth might know all that is mentioned, but might have given no proof whatever that he possesses

reasoning powers, or a capacity for discerniug between a truth or a fallacy, or sufficient discernment to perceive, that if he allowed a camp to be undefended, an enemy capable of rapid movement would probably massacre the army in such a position.

CHAPTER X.

Entrance Examinations—Former Examinations—Object of the Academy—Practical Knowledge Necessary—Useless Theory—Zulus' Caution—Rorke's Drift.

In the "General Regulations for the Government of the Royal Military Academy," it is stated that, "The Royal Military Academy is maintained for the purpose of affording a preparatory education to candidates for the Royal Artillery, and Royal Engineers. This education will be chiefly technical, and will in no obligatory subject be carried beyond the point useful to both corps alike."

The course at the Academy which is obligatory consists of:

1. Mathematics, including a thorough knowledge of plane trigonometry; practical mechanics, with the application of mathematics to machinery.
2. Fortification, field and permanent; such a course as is suitable to cadets qualifying for the Artillery, and the requisite amount of geometrical drawing.
3. Artillery; such a course as is suitable for cadets qualifying for the Engineers.
4. Military drawing, with field sketching and reconnaissance.
5. Military history and geography.
6. French or German at the student's choice.
7. Elementary chemistry and physics.
8. Drills and exercises.

In addition to this obligatory course, in which a cadet must gain half marks, he will be allowed to take up certain voluntary subjects, viz.:

1. Higher mathematics.
2. Higher portions of fortification.
3. Any one of the following languages: German, French, Italian, Russian, Spanish, or Hindustani.
4. Free-hand figure or landscape drawing.
5. Higher chemistry.

Very lately Latin and Greek were included in this voluntary portion; but the last regulations show that the importance of classics does not seem to be recognised at the Academy, and they have consequently been cut out of the voluntary subjects. How much longer they will be allowed to count for entrance, as much as mathematics, will be an interesting question.

When we remember that a cadet is only two years at the Academy, the list given appears to contain quite as much as could be acquired during that time. It must be remembered, however, that a candidate who comes up for admission to the Academy, may be within a few weeks of eighteen years of age, and if he has been trained for the Academy, nearly all the subjects mentioned in this list ought to have been thoroughly mastered before he enters the Academy. Forty years ago, when cadets joined the Academy at from fifteen to sixteen, they had at eighteen gone as far in mathematics as statics, dynamics, projectiles, conic sections, and the differential calculus. They had at that age

gone through a thorough course of field and permanent fortification, and practical geometry, and had passed in those subjects mentioned in the above list. At the age of twenty, I was commanding a half battery of Artillery on the frontier of the Cape Colony, when we were at war with the Caffres; and although crammed with the various subjects mentioned above, I found I had not been educated as a soldier, as I knew nothing of those important practical details which were of daily occurrence. The course of instruction at the Royal Military Academy has not consequently been extended and improved, as some outsiders erroneously suppose, but it has in many cases gone back; for, considering the age at which cadets formerly joined the Academy, and the age at which they gained their commission, they possessed a larger amount of knowledge than youths of similar age at the present time.

I have remarked, at a preceding page, on the number of marks which could be gained by a candidate for admission to the Academy, who took up no subjects other than those requiring a mere capacity for cramming. The preliminary examination has allotted to it 2,000 marks for mathematics, 500 for English composition, and 300 for geometrical drawing. Out of the 2,000 marks for mathematics a candidate ought to gain from 1,200 to 1,800; out of the other two subjects certainly 500 ought to be gained, making from 1,700 to 2,300 marks. Without touching on any further mathematics, a candidate at the subsequent examination ought

to gain from 1,000 to 1,500 out of English literature; from 2,000 to 3,000 for classics; at least 1,000 for French or German; from 500 to 800 out of free-hand drawing; and at least 1,200 out of general and physical geography and geology. Thus a candidate possessing no great talent, and giving no proof that he possessed any power beyond a capacity for cramming, might thus gain out of the subjects named no less than from 7,200 to 9,500. On the 22nd July, 1885, a list was published of the fifty cadets who had successfully competed for entrance to Woolwich, and the marks gained by each. How these marks were gained is not stated: but the first on the list gained only 9,666; the second, 8,012; the third, 7,484; and the fourth, 7,113; whilst there were fifteen candidates successful who had gained less than 5,000 marks, the two last on the list having gained less than 4,300. It is not stated what age each candidate was at the time of the examination, nor whether it was his first trial; but, considering that the possible number of marks to be gained by a candidate were 4,000 in mathematics, 4,000 in classics, 2,000 in English, 2,000 in French or German, and 2,000 in experimental sciences or geography, in addition to 1,000 in drawing, making 15,000, in addition to the possible score of 2,800 in the preliminary examination, I cannot consider that such a total as 8,000 even, indicates any very great powers of memory or capacity for cramming, whilst such totals as 5,000 are not very remarkable. The candidates could have possessed a smattering only of the subjects

on which they were examined, to account for such very feeble results.

It is, therefore, a delusion to consider that the modern system of competitive examination brings to the Academy better men than were brought forty years ago, whilst the short and sometimes hurried course through which cadets are hustled tends to make their knowledge superficial, and, consequently, easily forgotten.

Referring to the object of the Royal Military Academy, viz., affording a preparatory education to candidates for the Royal Artillery, and Royal Engineers, one can best judge of the value of such preparation, by inquiry from officers who, when they first took up regimental duties, either in a garrison or camp, what subjects which they had learned at the Academy they found of great use, and whether they ever found themselves in positions where they did not know how to act, in consequence of no instructions having been given them, relative to such conditions during their military training.

We should obtain some curious information if we examined officers in this way. Every individual has not the same experience. Some may encounter difficulties from which others escape; but I can relate my own.

On leaving the Academy my course in mathematics consisted of arithmetic and algebra, plane trigonometry, statics, dynamics, conic sections, projectiles (maxima and minima), hydrostatics, roofs and arches, and the differential calculus. I had obtained ·75 in the ex-

amination of these subjects. I was familiar with every known system of fortification. I had drawn with every detail Vauban's, the Prussian, the Modern, Coehorn's, and others. I had constructed a defiladed lunette, and had calculated everything with regard to the Remblai and Deblai. I could translate French into English, and English into French, and the same with German. I knew perspective, and could copy a water-colour drawing so as to imitate the original very closely. I could make a military survey with the aid of the compass, and, like most youngsters who have been thus trained, I fancied I knew a great deal. Upon joining at Woolwich, I was almost immediately given charge of a detachment of about thirty recruits. These I had to pay, and was responsible for their discipline. What the pay of these men was, how often they were to be paid, what deductions were to be made for their messing or regimental necessaries, or what course to pursue with a man who was in debt, I knew no more than I did about the management of a wholesale warehouse. Three months after joining at Woolwich, I was ordered to embark in a lighter at the arsenal with forty-five men, and drop down with the tide to Gravesend, and there embark on the freight ship *Senator*, and proceed in her to the Cape. When we reached Gravesend it was dark and snowing, the month being February, and there were no signs of our ship. After inquiry I found that, through a blunder of the authorities at Woolwich, I had been sent on a fool's

errand, as the *Senator* was in the London Docks, and would not be at Gravesend for three or four days. We had with us only food for the day's lunch, as it was supposed we should have reached the ship in two or three hours. The question was what to do with my men. Nothing that I had learned at the Academy bore on such a question, and the veriest dunce knew as much what to do as I did, as far as preliminary training was concerned. I had heard that billets were given to soldiers when no barracks were available; but whether I could march my men into Gravesend and order people to afford house-room, I did not know. Luckily I remembered that Tilbury Fort was opposite Gravesend, and on visiting it I found several empty rooms. A sergeant of the old school, who was in charge of the place, told me that he had no orders to admit my men, and stated that under the circumstances he could not give up possession to me. I settled the question by telling him I would take all responsibility, and ordered him to supply coals and bedding. How willingly I would have exchanged my knowledge of the differential calculus, for a little information as regards the details of the accounts of the gunners and drivers, or how to proceed with regard to billeting men, when unexpectedly left in a strange place.

During the voyage to the Cape the sailors mutinied, and refused to obey the captain; the ship was a freight ship, and consequently not much discipline on board.

The captain called upon me to assist him in quelling the mutiny and putting the ringleaders in irons. What were my duties and what my responsibilities? Suppose the sailors offered resistance to me, and in a scuffle I wounded a man with my sword, was I guilty of a grave offence? Again, I would willingly have exchanged my knowledge of conic sections for a little practical information as regards the duties and responsibilities of an officer in charge of troops on board a freight ship.

After a few months in Cape Town I was ordered to the frontier, and found myself in charge of two guns equipped as horse artillery. A patrol against the Caffres was ordered, and I commenced my experience of active service. Of the duties in camp, the precautions against an enemy such as the Caffres, even the best way to pitch and arrange the tents, I was as ignorant as though I had been trained for a parson. Vauban's systems I knew thoroughly; but none of these were suitable to an African bush, or to such an enemy as we were opposed to. I had long before this realised the fact, that all the knowledge I had gained at the Academy was of no practical use whatever as regards the everyday occurrences of my military life, and that I had to begin at the beginning to learn my profession.

I knew exactly the parabolic curve which a shot from my gun would trace as it travelled through the air, and the velocity with which this shot travelled;

but the clear air of the frontier rendered distances so deceptive, that when I guessed a range, and then fired a shot, I found the shot fell several hundred yards short of the mark aimed at. At that time I knew no method of rapidly obtaining an approximation to the range, the one item necessary to accurate firing. Some of my horses became very sick in consequence, it was supposed, of their having eaten something that was poisonous; there was no veterinary surgeon near, and what to do I did not know. Some years afterwards, having gained some knowledge on this subject, I rapidly cured two of my horses, which would certainly have died had they not been quickly attended to.

When travelling near the Draakensberg in Natal, my men being some miles in advance, I lost myself in some dense woody country, and darkness set in. I was several miles from where the troops were to camp, and there being no roads, but merely a beaten track, the chances of finding the camp were rather remote. Luckily, when on board ship, I had learnt the names of various stars, and where they were situated at certain periods of the night. As darkness set in, I saw Orion's Belt rising, and knew that the east was where this constellation was then situated. Making allowances for the apparent motion of this constellation, viz., 15° for each hour, I steered a southerly course, and at 11 p.m. came on my camp. All the science I had learnt at the Academy would not have helped me

under such conditions, whereas a little practical knowledge was of great service.

I have questioned officers who were in the Crimea, who had passed through the Indian Mutiny, who had served in the New Zealand war, who had encountered the Boers and Zulus, as to the practical value of the science they had learned at the Academy, and its application in the various campaigns through which they had passed, and the answers were such, that I am not at all convinced that the course of study at any of our military colleges is such as to be most useful to officers during their career in the service.

I must again refer to my text, viz., that when a pupil is compelled to study any subject, it should be for both, or one of the following reasons. It should be on account of the stock knowledge that is thereby stored in the mind, or because the subject cannot be studied without exercising the reasoning faculties, and thereby strengthening them, and consequently fitting them to perceive the most probable result which will follow certain acts, thus enabling the reasoner to guard against a disaster, or to ensure a success. The soldier or officer on service has rarely any opportunity of bringing into practice the higher branches of science; that which is of use to him is a practical, and thorough knowledge of very simple rules, and his greatest gift would be a facility for reasoning on the novel conditions which will always be presented to an officer on service.

I have already mentioned the proceedings which led

to the massacre at Isandula, and of the Prince Imperial. During three years I lived almost entirely with the Zulus, and I do not believe that any Zulu boy even, would have committed such absurd acts of folly, as led to these two disasters; yet none of these boys had ever passed through the Staff College, or the Academy.

I was once reproved by a young Zulu, for sitting beside him in the bush, and taking my seat, so that I faced in the same direction in which he faced. He immediately turned round and said: "Never sit down so that you both look in the same direction, because something may come behind both, and not be seen. Now you look behind me and I look behind you, so that we have eyes before and behind." Here was a simple and practical suggestion, the advantage of which is evident.

The absence of reasoning sometimes manifested in military operations plainly shows that those who directed these possessed no powers of reflection. Let us take another case which occurred during the Zulu war. The most stupid person, we should imagine would, after the disaster at Isandula, have realised the fact, that the Zulus had an admirable system of gaining intelligence, that they were brave and daring, and could move rapidly and secretly. Here are certain facts before us; let us now reason on the probable results which will follow such a proceeding as this.

A number of waggons loaded with food and ammunition are started, and sent into the Zulu country; they are escorted by a small force only, with two or

three officers. They have a long journey to make, and consequently have to camp at the end of a day's journey. Sentries are placed in the usual orthodox style, walking about in the same manner as they would do, if on sentry in the barrack square in England. They can thus be easily seen, and a Zulu knows where every sentry is placed, and can consequently stalk him without difficulty. The remainder of the escort lie down, and go to sleep. Now what is likely to happen? Can any reasoning man fail to perceive, that it is any odds in favour of this escort being surprised, attacked by overwhelming numbers, all massacred, and the waggons with their contents captured.

When the news reaches us of such a disgraceful blunder, it is accepted as one of the unavoidable incidents of war. No one of course was to blame, and nothing appears to have been learnt by the affair, other than that exactly the same proceedings must not be again repeated, as *experience* has shown that it leads to disaster. Was it necessary that experience should teach us what must occur, might not reason have perceived what must occur, and so extravagant an experiment been avoided? Then, again, who was it who ordered such a proceeding, and did not seem aware of what must be the result? Whoever it was ought to have been, if not tried for murder, at least sent home and employed in darning stockings, or breaking stones, or on some other similar occupation for which alone he proved himself to be fitted. When an

act of folly causes the loss of over one hundred lives, it seems strange that no notice is taken of it, and no one blamed.

It has been said by certain critics, that our "only general" has never yet encountered a formidable enemy, and has therefore given no proof of his great skill; but we may search carefully the details of the various campaigns in which he has been engaged, and we can find no instance where he has shown even an approximation to such carelessness, ignorance, and thoughtlessness, as was exhibited lately in South Africa and was mentioned above. And when we find that blunders have been avoided, we may be tolerably certain that this is not the result of mere chance, but is due to foresight and reason.

We have, in the defence at Rorke's Drift, an example of the application of the training at the Royal Military Academy. The Zulus had no cannon with which to knock down fortifications of even small resisting powers, consequently the full advantage of a fortified defence was obtained. An officer of Engineers, putting into practice his knowledge of field fortifications, holds his post with a few men, against an enemy fifty or one hundred times as numerous. This shows what might have been accomplished at Isandula, had the proper precautions been adopted, and the fatal results at that place show how useless is knowledge, if it is not applied practically at the right time.

CHAPTER XI.

Languages Requisite—Practice *versus* Theory—Sketching from Nature—Ranges shown by a Sketch—A Staff Officer—Mistakes of a Theorist—Incapacity to Reason—Spooring.

I HAVE already stated what I consider to be essential as regards the mathematical knowledge for a cadet. This knowledge ought to be gained by an average intellect at the age of seventeen; and the training after this should be in the direction of practical application. Geometry should be more encouraged than it is at present, and its application to various other subjects pointed out. Greek and Latin I would abolish, both for examination and during the course at the Academy. Each cadet ought to be able to speak, read, and write, either French, German, or Spanish; and now that so many officers of Artillery and Engineers serve in India, I consider Hindustani almost necessary. Why Hindustani should have been struck out from the Academy course, and Greek and Latin substituted, would puzzle those who were not aware of the influences at work to give occupation and profit to the numerous individuals who possess a knowledge of classics.

An officer on landing in India is in a most helpless condition, if he cannot speak Hindustani. If he procures servants who can speak English, he must usually pay them higher wages, and he is very liable to be cheated, in consequence of it being known that he does not understand the language, and cannot consequently make inquiries. The consequence is, that the officer hires a native to teach him, and begins to learn when he ought to know. If half of the time that is now given to classics, were employed in learning Hindustani, there would be no question, that the training at the Academy was such as to better fit an officer for his future career.

It is desirable, especially for an officer of Engineers, that every detail should be known relative to the construction of every system of fortification; but this ought not to be gained at the expense of practical knowledge, as regards field fortification. Each officer should be well acquainted with every available means of rapidly defending his camp, or of affording his men as much cover as is possible. He should be practised in the knowledge of ground, and should be made to select positions where he would camp, or even halt for a few hours, and should be taught what precautions ought to be adopted, under every probable condition, to make his position as safe and strong as the means at his disposal admittled of. The whole of these subjects might be classed under the head of reconnaissance. The rapid execution of such work is

not acquired in a day; great practice is necessary, and the powers of observation are called into active operation, and can be strengthened by being cultivated.

Military history affords an excellent opportunity for bringing into notice the causes of disasters and success in military operations. If the lectures, as are too often the case, give merely the dry details of the numbers engaged, the positions occupied, and the results of a campaign, the student does not learn much. When, however, the lecturer deals with the class of enemy to whom the army was opposed, and calls particular attention to the causes which led to success on one side, and failure on the other, he supplies information which the student retains in his memory, and can readily apply when the occasion occurs.

Military law forms no part of the study at the Academy. Can it be supposed that a knowledge of military law is not likely to be of any practical use to a cadet in after life, or is an officer supposed to acquire such information by instinct? An officer no sooner commands even a small detachment of men than he requires to know something of military law, in order to avoid making blunders. Every officer, however junior in rank, ought to know how to keep the accounts of his men, and how to pay a company. At present too many officers are so ignorant of such details, that they are entirely in the hands of their pay-sergeant, and sometimes suffer pecuniarily in consequence. Useful as is a knowledge of higher mathematics, or chemistry,

yet I would prefer possessing a knowledge of military law and military accounts. There is no greater error than that of overtraining in theory, to the sacrifice of practical knowledge. A jockey who is being trained to ride a race learns his business practically. He knows little or nothing of the anatomy of the horse, and would probably display his ignorance by showing that he did not know the difference between the ilium and scapula; but he can do his work in the saddle without such knowledge. Cook, the billiard player, tells a story, that he has been informed that to play billiards well, a man must know the co-efficients of the angles of incidence and reflection. He adds that he does not in the least know what is meant by these terms; but he will allow any mathematician to practise these angles on his table for a week, and will then give him 50 out of 100 and play him for a sovereign a game. That which is learnt by a cadet, to be of any practical use to him in after life, ought to be so thoroughly engrafted on his mind that he does not forget it. I believe that any individual who has thoroughly learned the multiplication table would not forget this, even though he did not do any multiplication during several years. Such, however, is not the case with a large majority of the subjects learnt at the Academy, or acquired by cramming at preparatory schools. Subjects are learned for an examination, and a few months afterwards are forgotten, leaving no useful deposit in the mind, because these subjects require too much mental digestion to be

absorbed into the brain during the time given for their study.

There has been an ever-recurring mistake made at the Academy relative to the amount of qualification considered necessary for an officer. One case occurred when I was a cadet. There being very few vacancies in the Royal Artillery and Royal Engineers, cadets were informed, that they would not be considered fitted for commissions, unless they had gone as far as differentials in mathematics, and defilade in fortification. We acquired such knowledge, and some twenty-two cadets were pronounced qualified. In less than a year afterwards, there was a great demand for subalterns in both corps. Cadets were then hurried forward. The course was so curtailed that many of these had only commenced trigonometry. Three examinations instead of two took place during the year, and forty imperfectly trained cadets were given commissions. Out of these forty scarcely one-half were enabled to remain in the regiment to attain the higher ranks, and many failed to reflect any credit on the corps. This delaying at one time, and hurrying forward at another, tend to make cadets think there is a great deal of nonsense in the asserted necessity for so much study; and if it be shown that these imperfectly trained cadets make equally as efficient officers as those who have been thoroughly trained, what is the use of so much schooling?

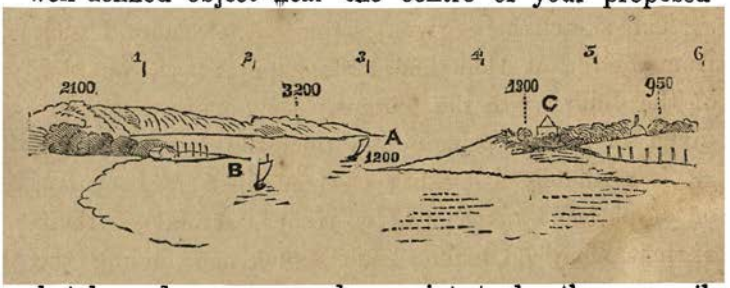
Artists, it appears are like poets, born but not to be made. Some individuals never can draw well, but

nearly every one can be taught to make a fairly accurate sketch from nature. I consider that a sketch made from nature, differs as much from a sketch made from a copy, as an original poem differs from a mere copy. To a soldier there is no great use in being able to copy a drawing, but there is great use in being able to sketch the outlines of country as seen from any fort, battery, or position. In connection with reconnaissance I taught cadets a simple method of making outline sketches, which required scarcely any artistic skill, it being a mere mechanical process.

Shortly after I landed in India in 1876 I was appointed president of two committees—one for improvement and re-armament of the fort at Allahabad; the other for similar work at the forts at Calcutta, Agra, Delhi, Gwalior, and other places. I found at these forts various tables of ranges; but the men at these forts being in some cases new to the locality, did not know the localities to which the names applied, so that the range tables were to them useless. At most of these forts I adopted a method combining sketching and range finding, which is very useful, and which I had commenced teaching cadets twenty years previously. As the details of this process may not be generally known, I give them for the benefit of the reader.

First select the extent on the horizon that you purpose sketching; about 60° or one-sixth of the horizon makes a suitable distance. Hold a pencil horizontally and at arm's length, and, closing one eye, move the

pencil its own length to the right, and thus measure from the left to the right of the proposed sketch on the horizon, counting how many times the pencil is moved. Suppose you find that the extent of your proposed sketch measures six pencil lengths, then divide the paper on which you purpose making your sketch into six equal parts, as shown in the annexed sketch. Then measure (with the pencil held at arm's length) on the horizon from the left point of the horizon to some well-defined object near the centre of your proposed



sketch, and suppose such a point to be three pencil lengths. Then under the mark 3 on your paper sketch the position thus found. A in the sketch shows such a position. Next measure with the pencil how far from the left of the sketch another noticeable position is situated, such as B. This point is slightly less than two pencil lengths from the left. Then mark on the paper just short of 2 the point B, and so proceed with any other remarkable and well-defined points, such as C, etc., and the relative position of these various objects is accurately transferred to the paper. Filling in minor details then becomes very easy, and making an outline

sketch becomes a mere mechanical process, requiring no artistic skill. The outline sketch having been made, write on it the ranges of various well-defined objects, these ranges being given in yards. This sketch should then be hung near the battery, and can be made use of by a perfect stranger, who does not know the name of any one of the localities seen from his battery, and, as may often be the case in a strange country, when the prominent localities have no name by which to identify them.

The sketch here given is one out of many I took from the fort at Allahabad, looking towards the junction of the Jumna with the Ganges. Only a few ranges are given to show the principle on which the sketch is taken.

Immediately a locality is fixed upon for a camp, or a position taken up by troops, several of these sketches may be taken; each sketch and finding the ranges could be completed in less than half-an-hour by one individual; and I believe a capacity to accomplish such a work is likely to be quite as useful on service, as would the power to mix colours and copy a water-colour drawing. I would include such a work as the preceding under the head of military reconnaissance, and it could be taught by the Instructors in that department. The method by which the ranges can be found will be dealt with shortly; a special chapter being devoted to this subject, as I have some very interesting and instructive information to give on this question, which may be of practical use to

officers desirous of bringing inventions or improvements into notice in the service.

There is no doubt that during the time cadets are at the Academy, they are hard worked; and as much instruction is given as is possible during the period. The only question is, whether this course of instruction includes those subjects which are most essential to an officer in garrison, or camp, during peace or war. Are the subjects taught those which an officer, to be efficient, must be acquainted with? And if not acquainted with, when he joins the regiment, he must learn by some means or other; for, until he has done so, he is incompetent to carry on his duties.

It is supposed that the best officers in the service are selected for the Staff, interest having of course nothing to do with such an appointment. Now let an individual consider what are the qualifications that he would desire in an officer who was on his Staff, in order that this officer should be of the greatest use. First he should be a very good rider, he should be able to write a good letter on any subject when the facts of this subject were put before him. He ought to be able to speak the language of the country in which he was employed, or if he could not speak the language, he should be intelligent enough to make himself understood by signs. He ought to possess that greatest of blessings, good health, so that he may stand fatigue, and bad climate, without

being under the necessity of going on the sick list on every occasion. Tact and judgment in dealing with both superiors and inferiors is also essential. Some Staff officers seem to forget they are not the general himself, and assume an amount of "side" which is most offensive.

A quick eye for country, a readiness of resource in novel conditions, and a thorough knowledge of the detail duties of a soldier; able to make a rapid reconnaissance and report, and an outline sketch. These are the qualifications which are most useful in a Staff officer.

Let us assume that we have the option of selecting such an individual, or of taking another who is an accomplished classical scholar, or who has gained the highest marks in mathematics, chemistry, physics, and fortification, but who rides only indifferently, has not condescended to acquire a knowledge of the detail duties of a soldier, and is not gifted with a hard constitution, and, as is usually the case, is not practical in matters of every-day occurrence. It may be urged that these men are born and cannot be altered by training. I should differ from such an assumption. Men are immensely altered by their training, as we may see by noting the sort of repetition of each other that men become, who have been trained in the same school or college. If the course of instruction at the Academy is such as to train a cadet for the special duties he will have to perform when he is an officer, then the course

is efficient and well chosen. If, however, a cadet's time is occupied in acquiring a knowledge of various technical matters, which, as an officer, not one in one hundred ever has a chance of applying to a practical purpose, and such knowledge is gained at the sacrifice of other, and more practically useful, though more simple matters, then the Academy course fails in what it assumes to do, as it trains men to be very learned in theoretical matters, and leaves them ignorant of what they are called upon to know during every day of their military life.

Very early during my service I saw two instances which were remarkable of a highly-trained officer being ignorant of common-sense, practical matters. An officer, who had distinguished himself by passing very high in the final examination at the Academy, was stationed at Natal, and was directed to adopt some means by which the camp on the Natal flat could be supplied with drinking water, in addition to the well which was in the camp. To accomplish this, the officer had a dyke dug from the Umganie river past the camp. The dyke was about four feet wide and three feet deep, the length of the dyke was about two and a half miles, the fall about five feet. When this dyke had been completed it was found that the fall was not sufficient to cause a flow of water along the dyke, the water, consequently, soon became stagnant. The worst business, however, was, that a Caffre one day did not step carefully over the dyke, but treading on the side pushed down the earth and

stopped up the entrance channel, thus preventing even the small flow of water, that might trickle along the channel; and this dyke was intended to enable the troops in the camp to obtain an abundant supply of fresh water, *in case they were besieged.*

The same officer built a powder magazine at the Point Natal; and I, finding the magazine always damp, sometimes so much so that the water ran down the walls, inquired from one of my non-commissioned officers as to the probable cause of this state. The magazine was not fit to contain powder, and was therefore useless. My corporal, who had been a bricklayer before he entered the service, smiled as he told me that the mortar used in constructing the magazine had been made from sea sand, consequently, just like the sea-weeds one uses as weather tests, it became damp on dry days, whilst the water ran down inside the magazine on wet days. This highly scientific officer was not aware that mortar made from sea-sand would always be wet; and it had never occurred to him, that the water, which he supposed would flow rapidly, with a fall of five feet in two-and-a-half miles, could be cut off in five minutes, by a Caffre pushing down some earth, and thus closing the aperture, and a garrison, which was dependent for water on this dyke, would have to surrender in forty-eight hours.

I have seen so many examples of weakness of intellect produced by overtraining, and an absence

of common-sense, apparently removed to make way for complicated theories, that I am much in favour of practical and useful subjects being taught, rather than the theoretical instruction so favourably received at most of our colleges. Another example I must give, showing how the overtrained mind seems occasionally to stumble.

A prisoner was tried for attempting to commit suicide by hanging himself. An officer, who was on the court-martial, and who had greatly distinguished himself at the Staff College, was asked by the president of the court-martial whether he considered that the evidence proved the prisoner guilty, or not guilty. The officer deliberated for a minute, and then said: "Let me see, is the prisoner tried for committing suicide, or only for trying to commit it." The president said that if the prisoner were tried for committing suicide, the fact of his being alive, and before the Court, would prove him not guilty. The officer, however, failed to perceive that he had said anything at all absurd or stupid, and argued that it was a very natural question to ask.

There is no doubt that overtraining does damage the intelligence, and prevents men from perceiving the most simple facts. Another example of such a result occurred in my own experience. A mathematician, whose reputation as such was European, was playing whist. I was looking over his hand. Two cards remained in each hand. The mathematician held knave

and nine of spades, the ace and king of spades having been played, and was second player. His right hand adversary led ten of spades. He hesitated a few moments, and then played the nine to the ten, holding his knave as last card. His left adversary held queen and small spade, and passed up the ten, which won; the queen of spades winning the last trick. I now asked the mathematician, why he did not put his knave on the ten, and head the trick, instead of playing his nine. The gentleman was most indignant at the question, and replied: "What nonsense you talk! What did it matter whether I played the knave on the ten, or the nine, as both my cards were of equal value when the ten was played?"

Seeing that the gentleman was angry at having his play called into question, and could not perceive the blunder he had made, I endeavoured to identify my mind with his, and to follow the course of reasoning which he had probably adopted when he hesitated whether he played the knave or the nine, on the ten led. "I have two cards in my hand, knave and nine. If the ten is out, then my knave and nine are of equal value; as my knave and nine are of equal value they must both be greater, or both less than a third card, so if any card is led, it cannot matter whether I play the knave or the nine."

Some such entanglement of ideas must have passed through the profound brain of this individual, or he never would have hesitated which card to play,

and have asserted that, as the cards were of equal value, it did not matter which he played.

The cultivation of the powers of observation is most essential. It is like collecting facts before we reason or theorise thereon. To pass through various countries, localities, or scenes, without completely observing, is the common method of proceeding, and some people do not know how little they have observed, until they are questioned about the country. The most careful observers I have ever met are the Zulus, who used to attend me on shooting expeditions. Nothing seemed to escape them; and I attribute this accomplishment to the fact, that their minds were clear from too much cramming, and were consequently, like a dry sponge, fit to take up anything presented to them. I have in one of my African books related some of the facts brought to my notice by these people; but two of these are so good that they will bear repetition. One was that they could tell a horse from a cow, or an ox, at an enormous distance, if they saw the horse rise from the ground, because, as they informed me, a horse rises on his front legs first, and his hind legs afterwards, whereas an ox does the converse. Probably nearly every person knows this; but I have not as yet met above one man in twenty who had remarked it. Another thing was that if you stand behind a cow, you perceive that the shape of the cow on one side, differs considerably from that on the other. Here again possibly the fact is known; but I never saw an artist's work, where the

two different curves were correctly drawn. The keen and close observation of these men, when examining the footprints of animals, is a lesson in itself, and can be applied to other matters. Every person knows that if a footprint of an animal, say an elephant, is seen on the ground, and the footprint of another and smaller elephant, breaks into a portion of the larger impression, then it follows that the smaller footprint was made last, and the smaller animal passed last. If we were to ask some mere mathematical astronomer, which were the oldest and which the newest of two adjoining craters in the moon, he would probably not be able to tell us. When, however, we see that the larger circle of one crater is broken into by the small circle of another crater, we know that the smaller crater must have been the last of the two formed. When we see also that a range of mountains in the moon has been broken into by a crater, we know that the crater was formed after the range, just as we should know that if the footprint of an animal was seen on a molehill, the animal passed after the molehill was thrown up; whereas if the molehill covered part of the footprint, then the animal passed before the molehill was raised. Of course such simple and self-evident facts would be comprehended at once by any individual who was clear-headed, and gifted with common-sense; it is merely when the sharp edge of the brain has become blunt and stupid by overtraining, that obscurity takes the place of clear-sightedness.

CHAPTER XII.

Range Finding—Scientific Committees—Rival Range Finders—Weak Tests—The Weldon Range Finder—Trained Men Required—Trained Marksmen—Knowledge of Ground.

In the preceding chapter mention was made of range finding being taught to cadets and officers, and I remarked that some interesting and instructive information would be given on this subject, which might be useful to officers anxious to introduce improvements into the service.

During the year 1848 I was stationed at Port Natal, and was directed by the captain under whom I served, to place a target on the opposite side of the harbour to that on which the battery was situated, and to commence practice at this target. At the date to which I refer, it was customary to fire a trial shot when commencing practice, and by such means to obtain an approximation to the true distance of the object fired at. Having in my possession a pocket sextant, it occurred to me that if I measured a base line near my battery, and measured the angles at each end of this base, formed by an imaginary line drawn from the target to the end of the base,

I should be able to calculate the distance from the target to the battery, and could thus obtain the range, and ensure my first shot being accurate, instead of merely a trial.

Not having a book of logarithms with me at the time, I plotted, on a large scale, the base line and angles, and measured on paper the distance of the target. The range was about 1,300 yards, and the first shot struck the target.

On reconsidering the method I had adopted, it occurred to me that if I made the base line form a right angle, with a line drawn from the target to one end of the base, I should then merely have to measure the angle at the other end of the base, and could then dispense with a table of logarithms; a simple table of tangents, enabling me to obtain the range. On testing this method, I found it gave very accurate results; and, after a little practice, I was enabled to obtain a range entirely by myself in from five to six minutes, and, if I had an assistant, the time required was usually not two minutes.

On my return to Woolwich from South Africa, I found that the system of a trial shot was still the fashion, in order to estimate the range; and these trial shots were frequently so much over, or short of the target, that two or three trial, and therefore waste shots, were necessary before the range was obtained.

Believing that finding the range by an instrument was cheaper, quicker, and more accurate than the trial

shot, I drew out a letter, in which I described my system, and forwarded it to the Secretary of the Ordnance Select Committee, which was then the tribunal to judge of such matters. This occurred in 1851.

Shortly after my paper was returned to me, with the remark written on it that I was thanked for my communication, but that the system of a trial shot was not likely to be improved upon by my "scheme."

In spite of this opinion, I always found the range first before I carried on practice at targets anchored at sea, or at localities where the exact range was not known; and I was convinced by results that to know the range accurately before firing, was better than to fire a shot at a guess distance.

When the Armstrong gun, with its accurate tangent scale, was introduced into the service, I thought that, when firing from coast batteries at a considerable height above the sea, the angle of depression could be obtained very easily to a ship at a distance, and the range ascertained. To do this I suggested that a spirit-level be placed in the bore of the gun, by which means the gun could be placed horizontally, by elevating the tangent scale, the angle of depression of a ship could be measured, and the height of the battery being known, a right-angled triangle was thus formed, the hypothenuse of which would give the range.

In localities where the rise and fall of the tide was considerable, and where the height of the battery consequently varied, I suggested three tables of ranges

being printed and hung up in the battery, one for high water, one for mean tide, one for low tide. As a guide for an officer in the battery, I suggested that a post be driven into the sea near the shore, and visible from the battery, on which post there should be marks by which an officer could tell the height of the tide, and consequently which range table to use.

I again submitted this system to the Scientific Committee of the Ordnance, and received the reply that it was considered that no scheme for finding ranges, would be likely to supersede a trial shot.

Being convinced that such ignorant prejudice would sooner or later be superseded by common-sense, I published in the papers of the Royal Artillery Institution a description of my method, and had range tables printed for the use of any officers, who might wish to make use of them.

Being engaged on other scientific problems, and finding that the obstruction principle was too powerful, I allowed the question of range finding to remain quiet during some years. I was then on the Staff at the Royal Military Academy, and one day I received a large official letter marked "confidential." On opening this, I found a letter from the Secretary of the Scientific Committee, in which he stated that a system of finding ranges had been discovered by an Austrian officer, and was applicable to coast batteries. That the method had been kept

very secret; but, through our officers at Vienna, it had been found out. The inventor had received a grant of money for his invention, which was considered very valuable, and I was asked to examine the process, and report whether it could be made available for our batteries, especially at Gibraltar and Malta. My reply to this question was simple. It was: "Look in the Royal Artillery Institution Papers of some years ago, and you will see the whole details of the method, which is identically the same as that which I there recommended."

Strange to say, no sooner was it discovered that the system was the same, than it seemed to be considered that it was of no value.

On second thoughts, I regretted my want of tact in this matter. Had I stated that the process was very valuable, and concealed the fact that it was my invention, I might have communicated with the Austrian officer, and when his system was adopted in our service—as it most certainly would have been —we might have shared the reward which he would have obtained as a foreigner.

It was shortly after this incident that it became known that the scientific branches of foreign armies were giving great attention to range finding; and the idea seemed gradually to dawn on our own authorities, that perhaps finding the range before firing, might be as useful as expending two or three shots for the same purpose. Lieut.-Colonel, then Captain Nolan,

who, as a cadet, had been under my instruction, proposed a plan for finding ranges by the aid of instruments, provided with large telescopes, and placed on guns. During the time that this officer was perfecting his plan, I assisted him in various ways; but I pointed out that the great defect was that the guns must be in position, and consequently exposed to fire whilst the range was being found; whereas, by my system, the range could be found before the guns came into position. As regards the relative accuracy, of course Captain Nolan's was the better; as the use of powerful telescopes, heavy instruments, tangent and slow-motion screws, necessarily gave greater detail. As, however, I found that with a competent assistant I could obtain ranges in from thirty seconds to one minute, and that the error rarely exceeded three per cent., I believed, that for practical purposes, my system was the better of the two; my instruments could be carried with ease by one man. They could be used after a few days' practice by any man who knew how to read; and ranges could be found whilst the guns were at a distance.

General Sir Lintorn Simmons, who was then Governor at the Royal Military Academy, having seen my system in practice, recommended that it should be tried before a committee. This trial occurred at Shoeburyness, and the results were so satisfactory that a second trial was ordered. At this second

trial the assistant who had been trained by me was unable to attend on account of sickness, and I had to employ as aid an officer who had received only a few hours' instruction, the results, consequently, were not as satisfactory as on a former occasion.

The report, however, of the committee, was so far favourable that I was ordered to Hythe to teach the Instructors there how to employ my method, and I was afterwards ordered to Aldershot to give instruction there.

A committee of three officers was also assembled at Woolwich in order to examine my system, and some amusing incidents occurred in connection with the proceedings of this committee, which are worth relating.

The trial consisted in the committee pointing out a distant object and requesting me to find the range. With a rope 100 yards long and some pegs, they then measured to the object. After I had measured two or three ranges I was informed that my instrument must be out of adjustment as all the ranges were about 100 yards short. As some of these ranges were not above 800 yards, and as I found my instruments in perfect adjustment, I knew the error must be in some other department. I therefore watched the rope measurement and found that a peg was placed at the starting-point, a second peg at the rope's end. The officer directing the rope gathered up each peg, and when he reached the

object, counted up his pegs, and gave the range as many hundred yards as he had pegs in his hand. This, of course, gave the range as measured 100 yards more than it really was. Upon pointing out this mistake to the officer, he could not perceive that there was an error, and suggested that the committee had assembled to judge and report on my system of range finding, not for me to cavil at their proceedings.

The second test adopted by this committee was to have two squads of men provided with ten rounds of ball cartridges. These squads were marched down to the rifle range, where the ranges were marked by pegs. One squad was supposed to guess the range, the other squad had their range given them by my range finder; each squad then fired at the targets, and the relative accuracy of the shooting of these two squads was then compared. I never could quite understand what this proceeding was intended to demonstrate. If it was to discover which of the two squads were the better shots, my attendance seemed unnecessary. If it was to test whether a man, entirely by guess, could know the range as accurately as it could be found by measurement, the labour seemed rather a puerile proceeding, and, as the locality was one where a man, by counting the pegs, could discover the distance of the target, even a guess was not made. I suggested that this experiment should be made in the country, where the ground was broken and irregular, and that one squad should guess, and

then the measurement should be made. But it was suggested that the men could not fire in the country, and so the experiment would be useless. What was in the mind of this committee seemed to be, to prove that men who did not know the range, could shoot just as accurately as those who did know it.

Having found that some men who were sent to me to be trained in range finding, could neither read nor write, and consequently could not read a vernier, I requested Mr. Simms, of the firm of Troughton and Simms, to construct three instruments, on the principle of the optical square, but to have the reflecting glasses so fixed, that the angles shown by these instruments should never vary. These angles were 84° 17′, 87° 8′, and 88° 5′. The object of these instruments was to enable a man to find his range, by merely bringing one end of his base and a distant object into coincidence, in the glass of his instrument, the range would then be ten times the length of his base if he used 84° 17′, twenty times if he used 87° 8′, and thirty times if he used 88° 5′. By using these instruments, the base would be variable, and the defect I found in it was, when the country was enclosed, it was difficult to obtain sufficient open space for a base line, to measure a long range. If the country were flat and open, the system worked well, but I did not from experience find it so efficient as employing a fixed base, and measuring the angle.

Having devoted much time and incurred considerable

expense in attempting first to force upon officials the advantage of range finding over trial shots, and secondly to introduce a simple method of accomplishing this result, I decided that the reward likely to ensue was not worth the trouble I had taken, and I therefore allowed the matter to drop.

When at Simla, in 1876, I was requested to give a lecture at the United Service Institution there, on reconnaissance and range finding. I did so, and described the two methods I had suggested, viz., to measure the angle with a fixed base, and to have a fixed angle of 87° 8′, 84° 17′, or 88° 5′, and a variable base. I had then in my possession, as I have now, the instruments with the fixed angles as constructed by Troughton and Simms, and showed them at the lecture.

My surprise may well be imagined when, some five years after I had given this lecture and some twelve years after I had taught this method of using instruments with fixed angles, I read a paper in the "Proceedings of the Royal Artillery Institution," in which it asserted that the discovery for which all Artillery officers had been vainly striving during many years, had at last been made by an officer in India, viz., a simple method of finding ranges, this method being, using an instrument with a fixed angle of 87° 8′ or 88° 5′. This method was discovered by Major Weldon, and the system is now known as Weldon's Range Finding System. The officers at the War Office were delighted

with the novelty, simplicity, and accuracy of the method, and have adopted it, and no doubt wonder how it was that no one ever thought of so simple and excellent a method before.

It cannot be expected that officers appointed from time to time, to occupy positions requiring great scientific knowledge, should be acquainted with the value or otherwise of various inventions brought to their notice. But it does appear peculiar that when an officer proposes an accurate method of arriving at a result, instead of a guess method, he should be informed that the guess is the better. When, however, a foreign Government show that they have decided that the guess is bad, then our own authorities make a great fuss and are most anxious to copy. Also it appears to be something more than mere stupidity, when we find that an invention proposed by one of our own officers and ignored by certain officials, is considered most valuable and important as soon as an outsider brings it forward a few years afterwards. In my own case I fail to perceive why the so-called Weldon Range Finder is now considered sufficiently valuable to be adopted in the service; when, fifteen years ago, I invented it, I was told it was not sufficiently good to be of use.

The absolute necessity of a means of finding ranges before firing is now admitted by all competent military men, who have now come to the same conclusion that I arrived at in the year 1848.

It required a long time and continued pushing to force on the routine mind this fact.

What we mostly require is thoroughly trained men, who should be kept in constant practice for range finding. I will undertake, with the aid of a competent assistant, to find ranges in less than two minutes, up to 3,000 or 4,000 yards, with an error not exceeding 3 per cent. A result sufficiently accurate for all practical purposes. The stereotyped idea of the official mind seems to be, that any man ought to be able to find ranges after a few days' training, and consequently that the system of finding ranges must be so simple, that the most stupid man could use it. I had actually sent to me at Woolwich six men, who were to be instructed in range finding, two of whom could neither read nor write. Great accuracy of sight is required in using instruments for range finding, and a certain amount of delicacy in handling the instruments. When men have been clumsy or careless, and have failed to find ranges accurately, the system has usually been condemned. We might as reasonably send our chronometer to the armourer sergeant to be repaired, and when his treatment failed, complain that chronometers were useless. Men ought to be selected for this important work, and should receive some small addition of pay or some distinguishing mark, to induce them to study and practice; or a prize should be given annually to the most accurate range finders in

the army. This would be money well laid out, when we consider the cost of one shot from our heavy guns, which shot, if the range were not known, would be wasted. We have annually at Wimbledon an assemblage of marksmen, who, when the range is known, will put bullet after bullet into the target at even 900 yards. This performance is, after all, merely mechanical. On service an enemy does not usually stand still, and tell you his exact distance whilst the marksman arranges his sight for the shot. During many years, accurate shooting has been taught in the army, and our men shoot well at a target, the range of which is known; but what are the results when practical facts on service are presented to us? How many shots are fired, and what is the amount of killed and wounded? When old Brown Bess was used, it required about one hundred and eighty bullets to kill one man. Have we improved on this? What amount of ammunition was expended at Ulundi, at Majuba Hill, at Tel-el-Kebir, and at other places, and what was the proportion between shots fired and enemy killed. If our system of teaching shooting is what it ought to be, how was it that the Boers lost only some half-dozen men, if as many, in assaulting Majuba Hill, whilst not a man on our side could show his head above the ridge but what a bullet entered his forehead. Although I am only an indifferent shot at a target, yet I am prepared to back myself against the best marksmen at Wimbledon under the following practical conditions. A strange country, and a running

buck; the buck getting up suddenly, and unexpectedly, at distances varying from one to four hundred yards, the distance of course being unknown. The reason why I make this statement is, that when in South Africa as a youngster, and afterwards in India, I have fired from ten to twenty bullets a day at running game, and my eye became trained to estimate distances, and I often fired as I would fire an arrow, or throw a stone, not by drawing an imaginary line up to my eye from the object, but by practice getting the hand and eye to work together. Some excellent target shots have been with me on these occasions, and their bullets never touched fur or feathers. It might reveal some interesting statistics, if some member were to move in the House for a return of the ammunition expended during various engagements in which our troops have lately been employed, and the number of the enemy struck by these bullets. Let us suppose that 1,000 men are engaged, and each man provided with 60 rounds, out of which he expends 30 rounds. We should here have 30,000 bullets fired. If the killed and wounded among the enemy amounts to 500, we should know that our marksmen had been successful in hitting a man once in 60 shots. From the estimate I have been able to form from official returns, I do not think that the facts demonstrate that our marksmen have been as yet able to attain to such perfect shooting as this. The instruction which we give our soldiers in shooting, is based on the same erroneous system as that on

which we teach our cadets. We have certain conditions laid down, and these conditions are not similar to those which exist in practice. The cadet, or officer, overtrained in the schools, is generally more useless on service than the man who has had but little training. The skilled target shot I have no faith in, when he encounters moving objects at *unknown* distances, and has some minor incidents to encounter which try his nerve.

"I can snuff a candle at ten paces," said Charles O'Malley, when asked if he could shoot. "But the candle has not a pistol in its hand," was the reply of Count Considine.

Great improvements have been made during the last few years, in the training of our soldiers to shoot at moving objects, and to fire with good aim more rapidly than was the case formerly. The greater the amount of training in this particular line, the more formidable will our small army become. It was the accuracy of shooting of our English archers which won many of our earlier battles; and although a bayonet charge may at times produce its effect, yet such a charge against marksmen, who are steady, and who can load rapidly, would be a fatal proceeding.

I was told by a German officer, who was present on the occasion to which he referred, that during the Franco-German War he saw a body of at least one thousand Frenchmen advance across some open ground towards a hedge and bank where a German regiment

was under cover. The French were allowed to approach to within about eighty yards when the Germans fired, the front rank first, the rear rank afterwards. When the smoke cleared off, about twenty men were seen standing out of the thousand.

Such steady shooting would render any approach to such troops impossible, unless the advance could be made under cover. Consequently a knowledge of ground, and a quick eye for the features of the country, are more necessary now that the accuracy of shooting is so much greater than it was formerly.

CHAPTER XIII.

Teaching *versus* Cramming—The Art of Teaching—Subtle Influences—Stereotyped Minds—Successful Instructors—Rule of Three.

THERE are two essential items connected with education, viz.: How to teach, and how to learn. The teaching is the business of the Instructor only, but to know how to learn is also a part of his work, whilst the learning only appertains to the pupil. It seems to have been assumed that if a man knows any subject he can teach it, and that no other qualification for imparting instruction is necessary than a knowledge of the subject by the teacher. As a boy I had a very early experience on this question. In my "Woolwich Cadet," I gave a description of a cram school at Woolwich. This description was accurate in every particular. I was nearly a year at this school and learned very little, because the system of teaching there adopted was merely one of cramming. It was attempted to cram the brain with knowledge, much in the same manner as food is forced down the throat of a turkey. The brain could not digest this, and actual stupidity in many cases was the result. I was

then transferred to the house of the Rev. Thomas Grose, at Trinity Square, Tower Hill. This gentleman adopted an entirely different course, and imparted his knowledge slowly, but surely; the result was, that during three months I learnt and thoroughly understood, twice as much as I had during a year at the Woolwich cram school.

Something more than mere knowledge of a subject must therefore be necessary, in order that a man should be an efficient Instructor; otherwise, how was it that during three months I learned more under one Instructor than I had during a year under another? Having had fifteen years' experience as an Instructor, and having watched the results of various systems of teaching, I may claim some practical knowledge on this question, and I now venture to give my conclusions thereon.

Upon commencing to teach any subject, the first elementary principles should be slowly and clearly explained. The Instructor should always bear in mind the difficulties which he experienced when he first began learning this same subject; and should endeavour to place himself in the same mental condition as that of his pupil. When he finds that the pupil is puzzled, or is slow to comprehend the explanation given to him, instead of at once by words or actions indicating that you consider him very stupid, endeavour to persuade him that his difficulty is a very natural one; that, when you were learning the same

subject, you at first experienced exactly similar puzzles; but that you soon found by patiently considering the case, it was very simple; and that sometimes that which appeared most complicated one day, seemed on the next day so simple, that you were surprised, when you understood it, how it was that it ever presented any difficulty. The perturbation caused in the mind of the pupil by the difficulty which he first encountered, will, during this explanation have ceased, or at least will have calmed down. He is then in a more fit condition to receive mental food, and to digest it. Inquire where the difficulty exists which has puzzled him, and listen carefully to his explanation; immediately he starts a false conclusion, stop him, and point out where this exists, and if this one error does not explain his difficulties, take the problem, or question, and work it out before him, explaining every step, as though you yourself found all the difficulties which he had encountered. Then make him rework the problem, and ask him the reason why he adopts each step. Having once taken this trouble with a pupil of average intelligence, you have done more to *educate* his mind than you would have accomplished had you crammed him with twice the amount of knowledge which was imparted by this one problem. One of the great and common defects of the mind which has been taught, but not educated, is to rush into a conclusion, and to be unable to follow out a simple course of reasoning, or to

continue this reasoning through continued and successive steps. If a man has to walk a mile he must do so by taking step after step, he cannot by taking one stride reach the end of a mile. It is the same in regard to reaching mental distances; we must advance step by step, and it is a common defect of those entrusted with instruction, to forget how many steps they had to take, when acquiring a similar knowledge, to that which they are endeavouring to impart, and to expect the pupil to make strides of ten yards at a time.

Immediately a pupil is puzzled, his mind appears incapable of receiving or retaining any information; and as the mind is usually inclined to be lazy, the pupil shirks grappling with a difficulty, and either gives it up, or attempts to get over it by some means other than that of understanding how to overcome it. I have often been surprised to find how utterly ignorant of some very elementary portion of a science some of my pupils have been, when they came to me to be instructed in the higher branches of the subject. I once had an officer come to me to be taught the higher branches of astronomy; he told me he had studied for some time the general principles, and understood all these. From past experience I had my doubts whether he did understand these elementary matters, and on questioning him I found that, although he knew various terms used in astronomy, he had the vaguest ideas as to what they

meant. At first he seemed rather indignant at my setting him to work to learn and thoroughly understand the meaning of right ascension, and to then explain it to me by aid of a diagram, also to explain to me how the right ascension of the sun varied during the year, and why; also to point out by aid of a diagram the difference between celestial longitude and declination. After a time he admitted that his knowledge had been scarcely skin deep. Another officer had been instructed in military surveying during more than six months, and yet could not make a sketch with any approach to rapidity. When he had worked with me during three days he made the uncomplimentary remark, "What a fool F—— must have been?" F—— being his former Instructor. The method I adopted with this officer was to make the sketch in his presence, explaining why I took various angles, why I selected certain objects, and proceeded as though I were learning what to do. I found that his former Instructor had given him very learned lectures, and explained how to get over some of the unusual difficulties which a sketcher may by chance occasionally encounter, but had neglected to teach him how to make use of the most simple rules in sketching.

I have noticed a remarkable circumstance occur in connection with instruction, which has been repeated so often, that it would appear as though some subtle personal influence were at work, and that this could

act on certain individuals. Most people have experienced the stupidity producing effect which some people's presence has on them, and the clear-headedness which others seem to produce. The special effect to which I refer is as follows. A cadet has been given a problem to solve; he has worked at this, has committed some mistake which he has failed to perceive and has become thoroughly puzzled; again, and again, he has gone over his work, but could not detect his error, he has then come to me and stated his difficulty. Instead of at once examining his work, I have requested him to place this on the table beside me, and have told him the problem was all right, and probably he had made some very simple mistake. I have then waited a few seconds whilst I tried to imagine what style of mistake he was most likely to make. Before I examined his work the cadet has suddenly said, "I see what I have done, I have looked out a sine instead of a tangent, or I have added together nine and seven and made it twelve," or some other error had been committed of as equally a simple nature. This discovery of the error has been made so often when a cadet, who had during an hour been puzzling over it, came up to me, that I was induced to believe there might be something in personal influence other than is recognised in orthodox science.

Whilst there may be systems laid down for instruction which will work well as a general rule, yet

there is so much mental individuality about young men, that no one system will be successful with all persons. To discover this peculiarity is one essential business of an Instructor, because, unless he do so, he will always have a certain amount of failures among his pupils. Every Instructor, after an extended experience, will have found that there are certain classes of mind which present exactly similar peculiarities. The errors of judgment, and the mistakes made by one member of this class, are repeated by another during the next half-year. Many persons, especially when young, are under the delusion that what they say, and do, is original; those who have had large experience in instruction, know that originality is the rarest attribute of youth. Even men are usually copies of each other, especially when they have been trained under similar conditions. The repetitions that I remarked among cadets were most amusing. In every class there were usually two cadets who would never rub up any Indian ink for themselves; but when they had to make drawings with this ink, they would wait, until some cadet near them had prepared his ink, and they would then fill their pens from their neighbour's stone, every time they required to do so. Strange to say, three half-years out of four, the cadets who possessed this peculiarity, occupied the same relative position in their respective classes, and, as might be expected, were very near the bottom. There were other cadets who

never had their note-books, or book of logarithms, in the Academy when they were required, but were compelled to go to their rooms to fetch them. Others would never make notes during a lecture, but would depend upon those made by another cadet. In every class there were two or three cadets who, when sent out surveying, would invariably leave their work, and adjourn to a certain public-house to have a smoke. So stereotyped were these proceedings, that there was not the slightest difficulty in appearing round a corner just as the cadets were entering the forbidden locality, and thus catching them in the very act. Certain problems in the course of instruction were never understood at once by from three to six cadets; and when a lecture had been given, these cadets would always come up to ask questions. After a time I knew so well exactly the reason for these cadets coming to me, that I used to anticipate their questions by saying: "I suppose you want to have it explained why so-and-so is the case, as you don't understand it." These cadets were surprised how it was I could know what question they were about to ask. They would not have been surprised had they been aware that cadets possessing the same class of mind had, each half-year during some eight or ten years, asked exactly similar questions when the same lecture had been given.

There is a most marked difference in the mental power shown by various individuals, and which can

only be tested when we put before them certain problems requiring reason, and perception, for their solution. There are some such which can never be comprehended by certain minds, any more than a dog could understand a problem in Euclid. I compare this defect to that exhibited by men who are asked to lift a weight which is too heavy for them. They may train and practise to any extent, and they will by such proceedings be able to lift heavier weights than they could lift previously, but they will never be able to equal the lifting power of a man gifted with twice their strength. I have heard of a very good test of the perceptive powers which may serve to illustrate the relative grasp of mind of individuals. Take two billiard balls, and roll these up the table. Every person who saw these balls rolling would be able to state that there were two billiard balls. He might under similar conditions be accurate as far as four, five, six, or even seven. When we advanced to nine, ten, eleven, or more, we should weed the observers, if we removed those who could not state the number before the balls touched the top cushion. I have found men who could not state correctly the number, if more than seven balls were started, whilst I have heard it stated that one gentleman celebrated for his powers in arithmetic could tell accurately each time up to twenty.

Errors in reasoning from certain data are repeated in a stereotyped form by certain minds. Those accus-

tomed to tuition can predict these errors, when they know the class of mentality with which they are dealing. We find these errors committed exactly in the same manner under various conditions; they are made in mathematical problems, in a game of chess, at a rubber of whist, and in the every-day occurrences of life, and seem to be as much a defect of the mental machinery, as a hobble in a walk, or awkwardness in the hand.

To be successful as an Instructor it is necessary that the peculiarity of the individual should be quickly discovered. Some minds appear very slow to receive impressions, but these, when once they do receive them, rarely forget what has once entered the brain. Those who very rapidly learn a subject, not unusually as quickly forget it, especially when it is one not requiring reason for its solution. Those minds which have either a natural tendency to reason, or have been trained to reason, rarely if ever fail to apply the same system of reasoning, and consequently never forget this system; it is those subjects which depend on memory alone, which appear to be mere lodgers in the brain, not permanent tenants.

The first important step which an Instructor should take, is to compel his pupils to reason, and prevent them if possible from abiding by mere rules. There are certain rules connected with every subject, which rules have been established in consequence of the reasoning which has been brought to bear on certain

well-established facts. To merely accept these rules without either inquiring, or reasoning as to why they have been adopted, tends to a display on many occasions of an amount of stupidity, which would be considered incredible, did it not occur.

Most persons must remember the case of an officer engaged with the enemy in rifle-pits in the Crimea, who finding his men running short of ammunition, sent to the officer in charge of the ball cartridge, for twenty rounds for each man. The reply of this officer was, that a demand for ammunition must be made in writing on foolscap paper quarter margin, otherwise he could not supply it.

However much we may teach, we yet fail to educate unless we train the mind to reason. When once a person is accustomed to reason (not to argue), then he begins to gain power for dealing with the novelties, which will assuredly come before him in his course through life. In arithmetic, I consider that double rule of three gives the best exercise in reasoning of any branch of the science; some such question, for example, as the following:

Suppose 30 men drink 6 barrels of beer containing 18 gallons each in 10 days, how many barrels of beer containing 9 gallons ought 20 men to drink in 15 days, supposing the men to drink the same quantity daily?

We have here a question which presents no difficulties if we take each portion separately. First, we wish to know how many barrels of beer are required, so we

place the number of barrels given as a comparison to the right, as shown below:

Barrels.
6

We then say, if 30 men drink a certain quantity, will 20 men drink more or less?—and our reason tells us that 20 men will require less than 30. As we multiply the second and third terms together and divide by the first, we must, in order to obtain less than 6, multiply 20 and 6 together, and divide by 30; and to "state," as it is termed, this problem in its first stage we write:

As 30 is to 20, so is 6 to number of barrels required for 20 men if all the barrels were the same size.

We next say, if a certain number of men drink a certain number of barrels of beer containing 18 gallons, would they require more or less if these barrels contained only 9 gallons? Of course more barrels would be required, so the next statement would be 9 to 18. We next deal with the days and ask a similar question as follows: will more or less barrels be required if we substitute 15 days for 10? Of course more will be required; the whole statement will therefore stand as follows:

Men.	Men.	Barrels.
30	20	6
Gallons.	Gallons.	
9	18	
Days.	Days.	
10	15	

We then multiply 20 by 18, the product by 15 and 6, and we divide this result by 30, multiplied by 9 and by 10, and we should find 12 the result, giving us the number of 9-gallon casks required. If we were to ask a person who had not been accustomed to reason, whether he could solve the preceding problem, he would probably say he could not, unless he knew what is called double rule of three. Ask him which would last longest, an 18-gallon cask or a 9-gallon, and he would probably think it absurd for you to ask so simple a question; again, if you inquired of him whether if you found a certain number of barrels of beer last 10 days you would require more or less to last 15 days, he would again consider the inquiry almost foolish; but the double rule of three is nothing more than this, and ought to be taught in a couple of days to any boy or girl endowed with common sense, yet I have known instances where boys have been three weeks at this one branch of arithmetic, and have yet been unable to solve so simple a problem. Not long since I gave to a youth who had advanced a considerable distance in higher mathematics the following rule of three sum, which I extracted from the *Family Herald*, July 14th, 1883. The youth puzzled over it for half-an-hour and then did the question incorrectly.

If 5 barrels of beer costing £9 last 24 men for 30 days, how many barrels at £2 a barrel will be required for 36 men for 24 days? Here we have a

similar problem to that last given, and as barrels of beer are required in the answer, we can at once "state" the question as follows: 36 men will require more barrels than 24 men; there will be more barrels at £2, than would be required at £9; and for 24 days, fewer barrels will be required, than for 30 days; so that we have the following as the form of statement:

Men.	Men.	Barrels.
24	36.	5
£2	£9	
Days.	Days.	
30	24	

By reducing the high figures in equal proportions we have:

Men.	Men.	Barrels.
2	3	5
£2	£9	
Days.	Days.	
5	4	

If we multiplied 5 by 3, 9, and 4, and divided by 5 multiplied by 2, and again by 2, we obtain 27 as the answer. But we can cancel the 5 by which we multiply, by the 5 with which we divide, and the two 2's cancel with the 4, so that we merely have to multiply 9 by 3 to obtain the correct answer.

I maintain that any youth who has passed through his course of arithmetic, and cannot at once solve

such a question as the preceding, and without hesitation, has never had his mind educated, nor is he capable of reasoning; he has learnt rules, and has forgotten them; but the principal object for which all this teaching has been given, has not been attained, for the youth has not learnt, or acquired the power, to reason. Had he done so, such a problem would not give him the slightest difficulty.

It is the business of the Instructor, to watch carefully that the pupil's knowledge of the elementary portion of the subject is thoroughly sound; should there be a weak portion, all that is built on this is unstable. It would be like building a tower on an unsound foundation, the tower, sooner or later, must tumble down; and it is better to build a small tower on a sound foundation, than a high one with such a weak base that it will fall almost before it is completed to its intended height. It sometimes occurs that many profound men are quite unable to instruct, because their giant minds, cannot descend to the small matters requisite to be attended to when teaching. A great amount of trouble was given me, in consequence of such a condition existing in the individuality of a great mathematician. Cadets, who had been under his instruction, would come to me to be instructed in surveying and practical astronomy, and I found many of them unable to work out a simple problem in plane or spherical trigonometry; and I was compelled to teach these two subjects before I

could proceed to give instruction in their application. The cadets themselves informed me, that they could not learn from this mathematician, as he was too profound for them, and if they asked him for an explanation of any difficulty, his explanation, instead of making the case more simple, made it more complicated, and the cadets left his presence more puzzled than before.

It frequently happens, that men who may know fairly well some science, are quite unable to teach it. I once knew a gentleman who was employed as a lecturer to cadets, and although I was fairly acquainted with the subjects on which he discoursed, yet I failed to understand what his lectures meant, and I gained no additional information from attending them. My case was not singular, as I ascertained that none of the cadets understood him, and it seemed almost pardonable, that many of them used to go to sleep during his hour's lecture.

The greatest tact and judgment are required, to administer the right amount of information to each pupil; cramming the mind with more than it can bear, is as damaging as though we crammed the body with too much food. Only a certain amount of information can be digested each day. As soon as one is mentally tired, the brain refuses to take more, and instead of producing any benefit, the mind actually suffers, and becomes damaged, by forcing it to attempt to continue cramming. The absurd system of com-

petitive examination, which it has been supposed produces very clever, common-sense men, gives us not reasonable and practical men, but too often a set of overcrammed individuals, whose minds have been actually damaged by being overtrained, and whose bodily health has suffered by the unhealthy process which has been adopted to enable them to succeed at a competitive examination.

To learn any subject thoroughly, we must proceed slowly and calmly, reflecting over each step, and not attempting to hurry; immediately the mind feels tired, and it requires great exertion to compel it to concentrate itself on any subject, rest should be taken, and no further attempt made to proceed, until it has acquired renewed working powers. Sometimes even a few minutes' out-of-door exercise, and fresh air, will so invigorate the mind, that we can continue working without either difficulty or distress.

When one has been hard at work on problems, requiring great mental power for their solution, a thorough change of ideas is almost necessary. Adopt some course which shall keep these problems from reoccupying the brain; the lightest reading or conversation are beneficial, whereas, any proceeding which admits of the same subjects coming again, and again, into our minds is damaging. Whenever we find that we are dreaming and worrying at night, on subjects at which we have worked during the day, we may depend that we are doing harm, instead of good, by

overwork. When a problem has been too much for us, instead of continuing at it, a rest of even a day will frequently enable us to at once solve it when we again examine it; we seem to have gained additional power during the interval of rest, and at a second trial to solve it, are surprised that we failed at first.

CHAPTER XIV.

Imperfect Investigation—Brain Power Invisible—Subtle Laws—Unreasoning Minds—Stored Minds *versus* Reasoning—Authorities—East to West—Satellites of Uranus.

THE amount of knowledge possessed at the present day, as to the manner in which our brain works, and the actual power of the brain, is much the same as it was two thousand years ago. By what means an original thought is produced in the mind, how knowledge is stored in the brain, and retained there. The extent of power possessed by the brain, why this power varies, and how its acts, are subjects as completely belonging to the unknown, as they were in the darkest ages of the world. We know that there are some men who show great talent, who can easily acquire knowledge, whose judgment is sound, and whose conclusions are almost invariably correct; whilst there are others who exhibit the very opposite qualities. We know this from observation, not from any examination of the brain itself. If a surgeon were to have submitted to him for dissection, or microscopic examination, the brains of some half-dozen clever men, and of half-a-dozen fools, he would fail to discover any material

difference between them. Certain men have gone so far as to state that, on a closer examination of the brains of animals, no difference has been found between those and the human brain. The fact, however, exists, that there is a great difference between the intellectual powers of man, and of animals; and if no amount of dissection, or of microscopic examination, can detect this difference, it proves that the power, whatever it may be, is so subtle as to be beyond the discernment of the microscope or the dissecting knife.

I would give to a blacksmith two pieces of iron apparently similar in every particular. He might hammer them, bend them, look at them with a microscope, examine the fibre of the iron, and, finding no apparent difference, would undoubtedly declare they were identical. One of these pieces of iron, however, is imbued with a wonderful power. It can cause pieces of iron to rise from the ground, and attach themselves to it. Solid objects cannot stop this power; it passes through these, just as thought will pass through the walls of a house. Place the iron on one side of a sheet of glass, and a needle on the opposite side, and the power which the iron possesses, will pass through the glass, and will affect the needle. The bar possessing this power is a magnet, the other is not. The blacksmith can discover no difference between these two pieces of iron, any more than a surgeon can discover the difference between the brain of an animal, and that of a man; but the difference exists nevertheless.

The error in both cases of the attempted examination being the same, viz., that the presence of an immaterial and subtle power, has been searched for by rough, material, and consequently unsuitable means.

That man possesses a power somewhat similar to that of a magnet, may be easily demonstrated by experiment.

Some years ago, an interesting electrical toy was to be purchased, and consisted of a wooden box covered with a sheet of glass. In the box were several small paper figures, which would rise, and stand erect, when the glass had been rubbed with a piece of paper, on which amalgum had been placed. If instead of these paper figures, a piece of swansdown were placed in the box, this swansdown would stand erect, when the glass was rubbed in a similar manner. Whilst the swansdown was thus standing erect, pass the hand slowly over the glass at a short distance, and the swansdown would move sympathetically, crouching down as it were, like a zoophyte when touched by a material body. It was impossible that any draught of air could affect the swansdown, as the box was closed; the power indicated, therefore, was similar, in some respects, to that possessed by the magnet, and was so subtle, as to pass through glass.

Now, the power which a magnet possesses, can be imparted to several bodies of iron or steel, and the original magnet does not appear to lose any portion of its force, by so doing. May it not be that the human

mind, the power by which we think or originate, is of a somewhat similar nature—that, according to the amount of this power, so is the strength of the intellect? We can impart this to other minds, and, by association and influence, cause these other minds to become magnets, when previously they were mere dull pieces of iron. Dissect a brain, or examine it with a microscope, and this subtle force is not perceptible, any more than the magnetic property is visible in a magnet; so, say the blacksmith class of investigators, there is nothing in the brain that indicates a difference between the animal or man. Such classes of minds talk in an apparently profound way, about the gray matter in the brain, which they state may be the thinking power; but that which belongs to thought, is so much beyond the testing power of the mere dissector or material investigator, that he fails to make any discovery, or to advance one single step towards an elucidation of the mystery.

The influence of mind upon mind would cause even the dullest individual to suspect, that there was something more subtle than what is termed matter in connection with the action of the brain. Those who have had much experience in instruction, must have found how apparently mysterious have been the influences they have produced on, and the facility with which they have imparted knowledge to, some individuals, and the difficulties they have encountered with others, who, in most respects, seemed gifted with equally as clear a brain.

Recent investigations, especially those made by the

Psychical Society, have proved that there is some influence which one mind exercises on another, though there is no contact between the individuals. Any two persons may test this power in the following manner: "Deal out at random eight cards from a pack. Let one person close his eyes, and think intently of one of these cards. The other person, having looked over these cards, closes his eyes, and pictures in his mind each card. If the two persons are susceptible, one person will, after a time, feel impressed that one particular card is thought of, and he will select the right card. The odds in this case are seven to one against his selecting the right card. If, therefore, he try this, with fresh cards each time, he would be correct seven times out of forty-nine trials. I have tried this experiment hundreds of times, and not long since I was correct six times out of eight. When some persons attempt to think of a card, they seem to wish to prevent the other from discovering which was thought of, and do not give the experiment a fair trial. There are others who seem unable to produce any effect upon another mind. This result is what would be naturally expected. A magnet will produce no effect on a piece of brass; but the failure to do so does not prove that magnetism does not exist. One example of a magnet, causing a piece of steel to become a magnet, is a fact which no amount of negative evidence will disprove. Coarse, unreasoning minds may term such results coincidences, or chance results; but such classes of mind are as unqualified to

investigate subtle phenomena of this kind, as is a dog to investigate a problem in geometry. If, as seems more than probable, what we term thought, is not the mere movement of the brain, but is the emanation of some subtle power, similar in some respects to magnetism, the influence which one mind produces on another, though no words have been uttered, receives a simple explanation. From experience, I know that it exists. If a better explanation of such an effect has been given, I have failed to hear of it.

We have now arrived at an age when these subtle laws must be investigated on scientific principles. Ignorant prejudice formerly prevented such inquiries, and those who advanced beyond the low forms of material science, were accused of witchcraft, and were burnt at the stake. Opposition still exists; but it now comes in a less formidable array, and emanates from the rough, blacksmith kind of mind, which assumes that it is practical, and does not deal with visionary matters, and consequently attempts to ridicule, or ignore, the careful investigations of more competent men.

What could be more disgraceful to the name of science than the conclusions of the learned men of France, when they asserted that mesmerism was an imposition, and those who dealt with it charlatans?

The treatment of Galileo by the opponents of the earth's rotation, was not a greater exhibition of prejudiced ignorance, than was the proceeding of these French so-termed "savants."

The history of the progress made in science, and the discoveries which, from time to time, have been made known to the world, afford endless examples of the feebleness of intelligence, and the incapacity for reasoning which have been displayed by those who were assumed to be the most scientific men at the respective dates.

The key to this apparent mystery is, I believe, not difficult to find. First, such men have been taught, not educated; they have been crammed with stock knowledge, but have never reasoned. When, therefore, a novelty is presented to them, they at once examine their stock knowledge, and, if they fail to find anything in accordance with the novelty, they at once reject it. Now, any novelty must, in the majority of cases, be something which is in opposition to that which has been previously taught. The rotation of the earth on its axis, for example, was in opposition to the stock knowledge which had been taught during many centuries; and the men who had thus been crammed, could not reason fairly and impartially on a novelty, which was in opposition to all with which they had previously stored their minds.

When men with minds only stored attempt to raise objections against a novel truth, their feebleness and incapacity to reason is exhibited by their bringing forward the most ridiculous arguments. For example, if the earth rotated, all the water of the sea would be flung off into space, just as mud is flung off from a revolving wheel.

"The Deity lives in the Heavens, not on the earth; *therefore* he makes the Heavens rotate, not the earth."

Scripture, which is inspired, was also brought into the objections, and it was asserted that, as it was stated that He placed the foundations of the earth so firmly that it could not be moved, it proved that those who asserted that the earth rotated, were attempting to know more than the Deity.

All such arguments were utterly devoid of reason, and proved that the men who used them, were taught in stock knowledge, but were as incompetent to reason as any one of the lower animals. Men in the present day sneer and laugh at the ignorance of these objectors of a past age; but, given a novelty at the present time, in opposition to that which has been accepted as true, and the stocked but unreasoning minds of to-day would treat this novelty exactly in the same manner as new truths were treated three hundred years ago.

There is but little originality in the human mind. That which certain classes of mind did a hundred or more years ago, will be done by similar classes of mind to-day.

There are in mechanics two branches, termed statics and dynamics. Statics treat of bodies which never move; dynamics of bodies in motion. Varieties of the human mind much resemble these two branches of mechanics. There are statical minds overloaded with stock knowledge. These minds never seem to move. They acquire and gather more and more facts, until they

become filled to repletion, and can take in no more. All facts outside of their stock knowledge are rejected. To attempt to reason with such minds, is as useless as attempting to demonstrate a problem in Euclid to a horse.

The dynamical mind is capable of reasoning, and moves in accordance with facts presented to it. Give to such a mind two explanations of any fact, and it will accept that which is the more reasonable. Give to the statical mind the two explanations, and it will not accept that which is the more reasonable, but that which is most in accordance with what it has been taught.

Exhibitions of an utter incapacity to reason, are frequently shown in small matters, and I cannot give a better example than one which occurred to me not long since.

I was playing a rubber of whist, and the king of diamonds was turned up to my left. As soon as I gained the lead I led a diamond, a small diamond was played by the second player, and my partner played ace of diamonds and returned the queen, which was, of course, taken by the king. I at once concluded that my partner had forgotten that the king was turned up to his right; but upon asking him at the end of the hand why he had played his ace instead of his queen, he replied that he held only two trumps, and was anxious to get two rounds.

"But," I remarked, "you would have obtained two

rounds by playing your queen. Did you forget the king was turned to your right?"

"Of course I remembered the king was turned to my right," was his reply; "but I tell you I wanted to make sure of getting two rounds, and so played my ace."

This proceeding exhibited a mind incapable of reasoning, and I considered it so remarkable a case, that I mentioned it to another individual who played whist; what was my surprise, when this second individual said, that he thought my partner played quite right, as by putting on his ace, he made certain of getting two rounds of trumps.

Now, if men with similar classes of minds, were occupying official positions, where they had to decide on the merits or truth of scientific questions, we can with tolerable certainty predict the amount of value which their decisions would contain.

Whether the subject be one of trifling importance, or of vital interest to science, the decision of an unreasoning mind will be the same. If this mind has been fully stored with stock knowledge, it is more likely to arrive at wrong conclusions, when a question requiring reasoning is submitted to it, than it is if free from such information. The stock knowledge is only of use, if the case submitted for consideration can be replied to in consequence of some well-known facts either corroborating, or proving the question to be wrong. If any person in former times asserted that two sides of

a plane triangle were less than the third side, a geometrician from his stock knowledge could affirm that this statement was incorrect. When it is asserted that the earth rotates, and that this rotation causes the apparent movement of the celestial bodies, a geometrician cannot deny it; but as in former times he found it opposed to all his previous teaching, he rejected it, and brought against it arguments which, for folly and an absence of any reason, were examples of a feeble intelligence amounting almost to imbecility.

Whilst, therefore, a storing of information in the mind is not without its use, yet the cultivation of reasoning is of far more importance; and whilst in our examinations we keep so much to testing the stock knowledge of candidates, and do not demand more evidence of reasoning powers, so long shall we tend to encourage mere automatons to take a prominent place, with the false stamp on them of clever men, whilst those, whose intelligence is of a far higher order, are kept in the background.

When we consider that the object of education should be to strengthen the mind, more than merely to store it, how absurd it seems that even now, some four or five years, and sometimes more, of a boy's life, are devoted to acquiring a thorough knowledge of two obsolete languages. These are driven into his mind, and are learnt in a parrot-like manner, until the mind itself loses much of its original vigour, and seems to become almost incapable of reasoning.

I will now venture to give some examples, which have occurred in my own experience, of the mental peculiarities exhibited by those whose minds have been well stored, but have been totally incapable of reasoning.

I have already referred to the case where one of the first mathematicians of the day assured me that the heavier (not stronger) a cable, and the more vertically this cable descended into the sea, the less would be the strain produced upon it by the pitching of the ship. Ask any practical sailor, who does not even know the multiplication table, whether, when at anchor, and the ship began to pitch heavily, he would shorten, or pay out his cable. Any captain of a ship who shortened his cable instead of paying it out, would not be looked upon by his crew as a very competent man; yet I now possess a letter from one of the leading mathematicians of the day, in which he asserts that, if a cable is so light that it enters the water some fifty yards from the stern of a ship, the pitching of this ship will be more likely to damage, or break the cable than if it were so heavy as to descend vertically into the sea.

When instructing cadets or officers it was necessary to be thoroughly acquainted with every detail of the subjects taught. The age and previous training of these, rendered the system of instruction very different from that which is adopted with mere schoolboys. The latter are merely told what to do, and they must do it. The former have a subject explained to them, and, as was often the case, they would discuss the explanation with

you, bringing objections against that which to them seemed obscure; or, as was often the case, they would discover the weak points in a problem or a demonstration, and would bring these to your notice. If an Instructor did not thoroughly understand every item connected with the subjects he professed to teach, his weak points were soon discovered, and cadets would cease to pay obedient attention to his instruction, and officers would not care to attend his lectures, when they could obtain as much or more information, by reading a book on the science taught. If any individual wishes to test whether he thoroughly knows any subject, let him attempt to teach it, not to children or boys, but to men, who on other subjects are as well informed as he is, and he will soon discover whether he really understands that which he tries to teach, or whether he is merely veneered with knowledge.

One of the first cases that I will mention, and which, from the lesson to be learnt, may be of interest, was with regard to the sextant. In taking angles with the pocket sextant, it was pointed out that objects should be at a distance of at least a quarter of a mile, or the correct angle would not be obtained. Also, to adjust the sextant, the object ought to be distant at least a quarter of a mile, in consequence of what was called the "parallax of the sextant." To understand thoroughly what this parallax really was, and how it affected angles, I looked up the subject in Jackson's "Military Surveying," and in Heather's "Mathematic Instruments."

In these authorities I found it stated that the effect of parallax was to make all the angles taken to near objects less than they ought to be. Accepting these two statements without doubt, I explained to a cadet, with a diagram why this was. The cadet was not satisfied, and the next day came again, and asked me additional questions on the subject. In the meantime I had gone into the matter, and had found that both the authorities were wrong, and had missed the true effect of parallax. The problem was a purely geometrical one, and to solve it geometry must be used. The real effect of parallax in the sextant I found to be, to cause no difference in angles of about 30°, whether the object was distant or near. For angles less than 30°, the measured angle was less than it ought to be if the object was near; for angles greater than 30°, the measured angle was greater than it ought to be. Having constructed diagrams to prove these facts, I put the problem away for two or three days, and then, placing myself in opposition, endeavoured to find a flaw in it. I failed to find a defect, and was convinced it was correct. I then submitted this problem to Professor Bradley, who was Professor of Geometry at the time. He looked carefully over my demonstrations, and the next day returned me my paper, remarking that the demonstrations were clear, simple, and correct; but inquiring why I had taken the trouble to give so elaborate a demonstration to what was almost self-evident. I then informed him that the "authorities" had given another, and different

explanation of the effects of parallax, and that my demonstration was opposed to these.

"You are correct, and they are wrong," was his reply, when he had examined the two books to which I have referred.

Considering the subject one of some interest as a geometrical problem, I drew out a paper on the subject for the Royal Artillery Institution, and forwarded this to the secretary. During about a month I heard nothing of the paper; and on inquiring as to its fate, the secretary was very mysterious, and informed me that he could not then tell me anything about it.

A few weeks afterwards, the secretary informed me that my paper would shortly be published; and that paper is now in the volumes of the "Proceedings." It was some time before I learnt the interesting details connected with this paper, before its publication.

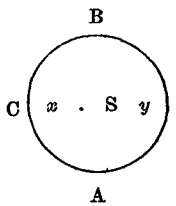
On its receipt, the secretary, not feeling himself competent to judge of the accuracy, or otherwise, of the paper, sent it to an officer of Artillery, much my senior, who had written about the sextant, and who was considered an authority. This officer returned my paper with the remark that I was "all wrong," and quoting as a proof of my being wrong, the two books to which I have referred. Fortunately, the secretary was of a somewhat obstinate nature, so he forwarded my paper to a distinguished astronomer, who, although he might be well acquainted with the transit instrument, and equatorial, had probably never used a sextant in his

life. This gentleman also stated that I was wrong, and returned my paper. The secretary was so obstinate, that he was not yet satisfied, but forwarded my paper to another astronomer, who, I trust, will pardon me for mentioning his name. It was sent to Professor Piazzi Smythe, of Edinburgh, who shortly returned the paper with a few remarks, in which he stated that my demonstration was correct. Professor Smythe's remarks were forwarded to the Artillery officer who had said I was "all wrong," and also to the astronomer who had come to a similar conclusion; both these gentlemen then withdrew their objections, one admitting that he had made a mistake, the other stating that he had been so busy, that he had not had time to thoroughly examine my problem, when he gave his first reply.

The paper was then published, and I had learnt my first practical lesson as regards the value of the opinion of authorities, whose minds have not been trained to reason, or who venture to give an opinion on a subject which they have been too busy to examine.

It was shortly after this event, when I was giving a lecture to some officers who had joined my class for Practical Astronomy, that I was explaining to them how celestial objects had their positions assigned by the terms right ascension and declination. I pointed out how the terms east, and west, did not apply in all cases to celestial objects, and if applied would probably produce confusion. To illustrate this fact I gave the following diagram and explanation. Suppose ABC the

orbit of the earth round the sun, S the sun. At midday the sun is south of an observer on earth; the east is to the left of the sun at midday, the west is to the right. Suppose A the position of the earth in March, and the sun is south at midday, and suppose a comet or other celestial body is seen to move rapidly from x, past S, and to y. This body moves from left to right, and would be said to move from east to west. Now turn this diagram round so that B is nearest to us, B being the position which the earth would occupy in September, and take the time as at midday, when the sun is south, and suppose the same celestial object moved as before from x to y; this object now moves from right to left of the sun, and would be said to move from west, to east, instead of from east, to west, as it did on the former occasion, when the body moved exactly in the same direction in both instances. East and west, therefore, are very dangerous terms to use, when we refer to celestial bodies.



I found on examining books on astronomy, that the danger of mixing up the terms, right to left, and west to east, did not seem to be quite recognised, as the terms were mixed, and used as indiscriminately as though they were identical, how if we notice the movements of our moon, we find that night after night, she has moved from our right to our left; and this as regards the Northern Hemisphere is from west to east. If, however,

we were in the Southern Hemisphere, we should perceive that the moon moved from left to right, instead of from right to left; but she would yet move from west to east. Consequently, if we state that a celestial body moves from right to left, we cannot say whether this is from west, to east, or east to west, unless we know from which hemisphere we are observing this body.

These facts having been explained, I was asked whether this confusion of the terms east and west would explain the mystery connected with the satellites of Uranus. In order to explain to the reader unacquainted with astronomy what this mystery is, the following explanation must be given. When the planet Uranus was discovered, it was found to be attended by at least eight satellites; these satellites, it was stated, moved in opposition to the movements of all the other satellites in the system, as they moved from left to right, *that is from east to west*, around Uranus, instead of from west to east, as the satellites of every other planet move. Now the axis of Uranus is not inclined at an angle of about 23° 27′ as is that of the earth, but is so much inclined, that it almost coincides with its orbit; consequently, we on earth are looking down on the south pole of Uranus when it is in one portion of its orbit; whilst, forty years afterwards, we are looking down on its north pole. When, then, we perceive the satellites of Uranus moving from left to right, it is a mere arbitrary assumption to assert that these satellites are moving from east to west, because, unless we can affirm

that the pole we are then looking down upon is not the south pole of Uranus, then the movement from left to right is from west to east, round Uranus.

This fact has been entirely lost sight of when the assumed mystery of a movement from east to west of the satellites of Uranus has been asserted. Even if one pole of Uranus were what we may term *below* the plane of its orbit, it does not follow that this pole may not be the north pole of that planet. To assert that the movement of the satellites of Uranus is in opposition to the movements of all else in the solar system, is an incorrect assertion.

Having prepared a paper on this subject for the Royal Artillery Institution, I read it and invited a discussion on the demonstrations I had given. Instead of a discussion on the facts or demonstrations, I was opposed by a gentleman who prided himself on his mathematical knowledge, and whose arguments were based on the assumption, that it was most improbable that my demonstrations could be correct, considering that all books on astronomy stated that the satellites did move from left to right, which *must be* from east to west. All my endeavours to induce this individual to reason on the problem were futile. He kept to the dogma that, from left to right must be from east to west. I endeavoured to induce him to imagine himself in the southern hemisphere, and to be facing the sun at midday; and I asked whether from left to right would then be from east to west. He replied, that he

avoided imagination and kept to scientific facts, and he could not admit that there had been any oversight or mistake made by mathematicians with regard to the satellites of Uranus.

My paper, however, was printed in the "Proceedings," and in modern astronomical books; the assertions previously made about the satellites of Uranus are being quietly omitted. Some unreasoning minds still, however, repeat the mystery, and fail to comprehend its solution.

We have these two cases as admirable examples of the incapacity—to reason—of minds which have been stored and taught, but not educated. A novelty is brought forward with regard to an instrument. This novelty is of course in opposition to what has been previously taught. The novelty is true, the former teaching incorrect; but when the novelty has been clearly demonstrated by geometry to be accurate, the taught mind cannot reason on the proof, but lapses into as complete a condition of feebleness, as is the normal mental state of the most uneducated mind. When, however, a distinguished man of science pronounces the problem to be correct, then the taught mind accepts the statement of this authority, though probably utterly incapable of reasoning out this proof. Something, however, may be said in favour of one of the two objectors to my proof, of the true effect of the parallax of the sextant, viz., that he admitted he had been wrong. The other gentleman sheltered

himself under the excuse, that he had been too busy to examine the problem. This excuse claims that, although too busy to give a correct opinion, he was not too busy to give a wrong one; and he seemed entirely to forget that to express a decided opinion, when, as he claims, he was too busy to examine the demonstrations, is a proceeding not likely to add to our respect for the judgment, or mental capacity of such an authority.

We have in these two minds no novelty; we have exactly the corresponding types of those, which asserted that the earth could not rotate; that all former authorities asserted that the earth was stationary, and the heavens rotated; that if the earth rotated, eclipses could not be foretold as they were foretold; and in fact, that the idea of the earth rotating was all wrong. In other works, we again meet examples of the mental imbecility produced by teaching and cramming, without educating the mind to reason.

CHAPTER XV.

Comets' Tails—Moon Half Illuminated—The Pole Star and Pointers—Tests of Reasoning—The Earth's Movements—Something not known—Unorthodox Objections—A Circle and its Centre—True Movement of the Earth—An Original Formula—Astronomy *can* explain Geological Facts.

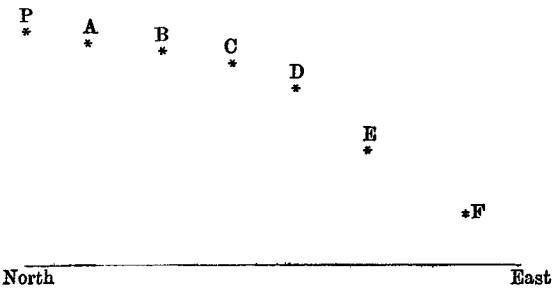
THE examples of mental peculiarities which have been brought to my notice during my experiences are, I consider, of such importance, not only to the progress of real science, but also as guides to those who wish to educate, cultivate, and strengthen the brain, instead of merely cramming it, that I venture to give two more instances, one of which is of great importance and interest to all those who wish to know something of the past history of the earth.

During the year 1861, I was staying in the New Forest, Hampshire, during one of my vacations. One afternoon, almost immediately after sunset, I observed a brilliant object in the sky. This object was only a few degrees above the northern point of the horizon. Knowing that no planet could be situated in this part of the heavens, I announced to my companion that what we saw must be a very large comet, which must be moving upwards, and with great rapidity, because no signs of it existed on the previous night.

When darkness set in, the comet presented a most brilliant appearance; the nucleus was near the northern horizon, and the tail stretched *in a straight line* to the zenith, and several degrees south of the zenith. As the evening passed, I made several observations on this comet, and noted how rapidly it was moving. Six hours after the comet was first seen, the nucleus had reached to the east—a movement due to the rotation of the earth; but the tail now stretched from the nucleus up to near the pole star and to the west; and this tail, instead of being straight, as before, was now *curved* like a scimitar. My companions, who were not acquainted with geometry, were astounded at this change, and seemed almost alarmed to find that the tail which, six hours before, had been quite straight, was now curved; and they imagined that the change from the straight line to the curve, was due to some movement in the comet's tail.

I informed them that no change had really occurred in the comet's tail, but that the effect was due to the rotation of the earth, this rotation causing us to see the comet's tail from a different position. To explain this fact I gave the following illustrations. Suppose five or six stars were seen situated one exactly beneath another, and all these descending in a straight line from the pole star to the northern point of the horizon, we should of course perceive these stars arranged in a straight line. Six hours afterwards these stars would have appeared to rotate round the pole, and would have passed to the

east, a change due to the rotation of the earth; but these stars would not now appear in a straight line, but would appear as shown in the following diagram; ABCDE, etc.,



being these stars, and showing in the form of an arch. When by the rotation of the earth these stars were exactly south of an observer, then they would again appear in a straight line.

As another illustration, I desired my companions to imagine they were on the equator of the earth on the 21st March, and to remember that on 21st March, the sun travelled over the equator. Then if they drew an imaginary line from where the sun rose, to that point in the heavens where the sun was situated at midday, they would draw *a straight line* from the eastern point of the horizon to the zenith. If, however, a person in England were to draw on the heavens an imaginary line, from that point of the horizon where the sun rose to the point at which it reached at midday, he would have to draw *a curved line*.

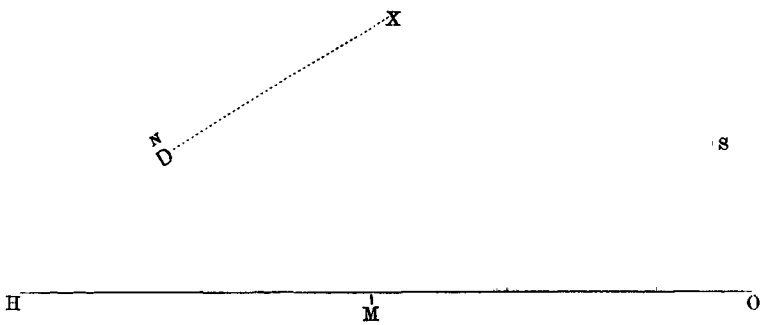
If now, the sun, instead of being a spherical body,

were 90° in length, and an attenuated body, like a comet's tail, it would appear to an observer at the equator like a straight line, one extremity being at his zenith, the other on the eastern point of the horizon, whilst at the same instant, it would appear to an observer in England like an arch in the heavens, one extremity being on the meridian at an altitude equal to the colatitude of the place of observation, the other extremity being on the eastern point of the horizon, the curve or arch, descending gradually near the meridian, and rapidly near the horizon.

One of my companions stated that he at once comprehended the problem, the other said he could not understand it; and although I gave several other illustrations of the fact, he was yet unable to perceive, why this change in the apparent shape of the comet's tail was due to the rotation of the earth.

There was here given an admirable example of the relative powers of the two brains; one could realise the two positions of the comet, and could perceive why the tail should appear straight in one part of the heavens, and curved in another part; the other could not grasp the explanation. I have frequently tried this problem as a test on various minds, and find that not one in ten is capable of understanding the cause that produces the effect. Strange to say, in Herschel's Astronomy, it is stated as a mysterious fact, that comets' tails are sometimes curved, sometimes straight; the reason why this is so, is not stated.

There are two other interesting facts depending on the same law which are not usually understood. It is known that the moon is illuminated by the sun. When the moon as seen from the earth is only half illuminated, we shall perceive the moon at about 3 p.m. in the south-east, and the moon appears tilted as it were, as shown below. HMO is the horizon, M the south point, N the



moon, and S the sun. A line drawn apparently at right angles to the dark line on the moon appears to be directed upwards in the direction of X, and not towards S, the sun. The cause for this is the same as that which produces the apparent bending of the comet's tail, for the line from the moon to the sun must be represented on the heavens as a curve, although it is in reality a straight line.

Another singular effect is that the two pointers which point to the pole star do not always point with equal accuracy towards the pole star. The two pointers, viz.,

α and β, Ursæ Majoris, point exactly to the pole star when they are slightly south of east of the pole; when west of the pole, they do not do so. The reason for this change is the same, and may be understood by a simple experiment. Take an umbrella, and open this, direct the handle of this umbrella upwards about halfway between a horizontal and a vertical position, place two pieces of white paper on one of the ribs of the umbrella, these two pieces being about six inches apart, and the piece nearest the stick about one foot from it. Place a third piece of paper about two inches above the stick, and on the umbrella. When the two pieces of paper are on the right side of the stick, they will appear to point directly towards the third piece. Then cause the umbrella to rotate so that the two pieces of paper are vertically one above the other. The third piece will now have moved round to the right, and the pointers will point to the left of the piece of paper, near the stick. Continue the movement, and bring the two pieces of paper to the left. This same movement will cause the piece of paper near the stick to be now below the stick, and the pointers will point to a part of the umbrella above the stick, and not directly towards the third piece of paper as they did at first. Now this is exactly what occurs with the pole star and the pointers, and the cause is as described above.

It is, perhaps, one of the most remarkable facts connected with the imperfect observation of men, that neither in ancient or modern astronomy, do we find any

mention of this fact; in reality, it has escaped observation, yet a most important law is dependent thereon. It appears still more remarkable that, on my calling the attention of an eminent astronomer to the problem, he informed me that he had never noticed any difference in the directness, or indirectness, in which the pointers pointed towards the pole star, and, he added, that as the stars never altered their relative positions, *he did not see how it was possible.* One can only reply to such a statement by the time-honoured remark, I do not say it *is* possible, I only say it occurs, and what is more, I can explain the cause.

I may here add that certain errors connected with lunar observations, which are known to occur, but the real cause of which has not only remained undiscovered, but unsuspected, is due to this fact relative to the apparent bending of comets' tails; to the illuminated disc of the moon not appearing at right angles to a line joining the sun and the moon; and to the pointers not always pointing with equal accuracy towards the pole star.

It is a singular fact to record, that my attention was first drawn to the illuminated disc of the moon not being at right angles to a line joining the sun and the moon, by a Zulu. This man asked me whether the moon shone by its own light, or by means of the sun shining on it. I told him by means of the sun shining on it. He replied this could not be so, for if the sun caused this light on the moon, then the line of darkness would

be up and down exactly, whereas it was not so, and he held up a circular gourd on which the sun shone, and pointed this out to me. At that date I could not meet his objection, and it was not until I had made astronomy a more complete study, that I was able to do so. But to find astronomers who have devoted years to the study of this science, and who have never remarked the facts, is certainly singular.

The question may be asked by some readers, What does it matter whether comets' tails are straight or curved, or whether the pointers always point with equal accuracy to the pole star? As regards the practical use of these facts, it may not signify much; any more than it signifies, whether the earth travels round the sun, or the sun round the earth. Every astronomical event could be foretold with equal accuracy under either condition. But these questions call into action reason, and are tests of the reasoning powers of the mind; a man who has never noticed the peculiarity, relative to the illuminated disc of the moon, or the pointers and the pole star, is one whose perceptives are dull, and who is not gifted with much observation, especially if he has ever given much time to the study of astronomy. So imperfect is the observation of some people, that on one occasion I saw in a well-known cartoon by a celebrated artist, the crescent moon drawn with the illuminated curve turned *away* from the sun. Again it might be asked, what does it matter whether the sun is ninety or ninety-five millions of miles from the earth, yet many thousands of pounds

have been lately expended on the expeditions for observing the transits of Venus, in order to obtain the distance with greater accuracy, and the result as far as we yet know have been "nil."

To ascertain a truth is apparently the object and aim of science; consequently every fact in Nature is worth observing and recording, although the mere business, or money-making man, may consider this truth of no value.

As a test of the capacity for reasoning, and of the powers of observation I have found these questions relative to the moon, the pointers, and comets' tails, most efficient; they are not even yet sufficiently well known to be among the subjects that are crammed, and, consequently, we can have examples of how the uncrammed mind deals with them; they occupy, relative to so-termed scientific minds, much the same position that an announcement of water becoming solid by great cold, occupies in the mind of the learned chief, who has never travelled beyond his own tropical country. He has never observed the fact. The chief, however, has never had the opportunity of observing the fact. The scientific man in Europe has had the opportunity of noticing these astronomical changes, and has usually failed to do so.

When giving lectures to officers relative to the movements of the earth, I have had questions put to me which indicated the feebleness of the mind of the inquirer; but I have also had questions asked which caused me to look closely into subjects, which I had been accustomed to consider settled "by authority."

One of these questions will now be dealt with.

The earth, according to the theories of Copernicus and Galileo, has three principal movements: first, a rotation on its axis in twenty-four hours; secondly, a revolution round the sun during the year; thirdly, a conical movement of the axis of daily rotation. This conical movement is made at a very slow rate, amounting to a change in the direction of the earth's axis of only about one degree in 180 years.

I had long taught this third movement in the orthodox routine manner given in the text-books, when I was asked, whether the other parts of the earth partook of this movement as a partial rotation, or as a mere swaying.

I could not reply to this question in detail, as I could find no mention of it in any work on astronomy. I therefore wrote to an astronomical authority, asking him certain questions about it. The reply I received at once convinced me that the authority had never reasoned on the subject, and knew no more about it than I did. I had, consequently, to tread my way where there was no beaten track, and to work out the investigation by myself.

I first found that the conical movement of the axis of a rotating body could be produced by giving to the body a second slow rotation round a second axis. It could be produced by swaying the rotating body without giving a second rotation. The effects resulting from the first cause, would be very different from those resulting from the second, although in both cases the axis of daily rotation would make exactly the same movement. The

other parts of the rotating body would, however, make very different movements. The two cases may be understood by the following diagrams. Fig. 1 we will suppose a sphere rotating rapidly round an axis NS. Whilst rotating rapidly round an axis NS, we will suppose this sphere to be also rotating slowly round another axis *xy*. When half a rotation round *xy* had been accomplished, the axis NS would occupy the position OP. A point A on the surface of the sphere, would by this half-rotation be carried to B, and the point B would be carried to A; the effect on this sphere being, that half a rotation had occurred round *xy*, the semi-axis of NS would have traced one-half of a cone, and the axis SN, if produced to the sphere of the heavens, would have traced half a circle in the heavens. We will now explain the effects of the swaying movement of the sphere.

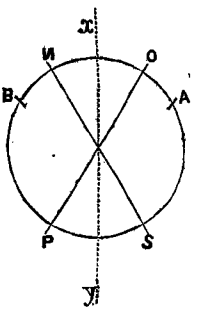


FIG. 1.

NS represents the axis of rapid rotation at a given date. After a long period the axis, in consequence of a mere swaying movement of the body, is carried to the position SN', the axis NS having thus traced out half a cone. A point B on the surface of the sphere, would by this movement be carried

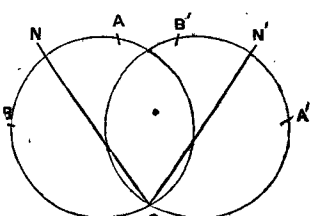


FIG. 2.

to B′, and a point A to A′. By this second movement the point B would not be carried round 180°, as it would be by the former movement, but the axis itself would in both cases point to exactly the same parts of the heavens during the entire conical movement. In the first case, there would be one entire rotation of the body round the axis of the cone, traced by the axis of rapid rotation. In the second case, there would be no rotation in the body other than the rapid rotation round the axis NS.

Having considered these two movements during several months, I inquired of a distinguished authority on astronomy whether the conical movement of the earth's axis was produced by a slow rotation round a second axis, or by a mere swaying movement of the earth, and what proof he could give me of the movement being that which he might consider occurred.

When I received his reply I gazed at it in astonishment, for it was to the effect that no matter whether the conical movement was produced by a slow rotation round a second axis, or by a swaying movement of the earth, the effects on all parts of the earth would be the same, and their relative positions as regards distant celestial objects would be identical in both cases.

I read and re-read this reply, and compared it with my diagrams and models, which proved the very opposite to be true, and I finally came to the conclusion, that if I had given such an answer to a class of officers, or cadets, I should not have dared to show my face to them again as an Instructor.

The conclusion I eventually came to, was, that this was a branch of astronomy which was not quite as well worked out as it might be. In fact, that the leading astronomers of the day knew nothing about it.

On finding that such erroneous and incorrect ideas prevailed on this one branch of astronomy, I devoted some years to the investigation of the results, which would take place, according as one or other of these movements of the earth really occurred. Any person possessed of reason, although not skilled in astronomy, must perceive that the exact locality at which a total eclipse of the sun occurred at a remote date, could not be fixed accurately, if it were not known whether the conical movement of the earth's axis was due to a slow second rotation round a second axis, or to a swaying movement of the earth. And if the erroneous belief existed that these two movements were identical, then the record of ancient eclipses would either be considered untrue, or some theory relative to the moon's movements would have to be invented in order to account for recorded facts.

It was not many months after I had found that the conical movement of the earth's axis could be explained by two very different movements of the earth itself, that I was giving a lecture relative to the annual changes in polar distance, which occurred to each star, and which change is due to the change in direction of the earth's axis. I pointed out how stars which were on the meridian at midnight in September, decreased their

polar distance about 20″ annually, thus showing that the earth's axis changed its direction to the amount of about 20″ annually, and towards those stars which were situated in that part of the heavens. This movement would of course produce a very small change in the polar distance of stars at right angles to those most affected. I then explained that this movement on the earth's axis was circular or conical, so that the axis traced on the sphere of the heavens a circle round the pole of the ecliptic as a centre. In a subsequent lecture I pointed out, that the extent of the tropics on earth was always exactly in amount the same as the extent of the arctic circle, and I showed by a diagram why this was so. I gave the geometrical diagram, showing why the angular distance between the pole of the heavens and the pole of the ecliptic, must be the same at all times as the extent of the tropics at those times.

After this lecture, an officer asked me whether there was not a record of the extent of the tropics having gradually decreased during the past two thousand years, and that this decrease was still going on.

"Certainly it is," I replied, "and it is called decrease in the obliquity of the ecliptic, and amounts to about 45″ per century."

"But," said my inquirer, "you told us the other day, that the pole of the heavens traced a circle round the pole of the ecliptic *as a centre,* and that the angular distance of these two poles, was the exact measure of the tropics and of the arctic circle. Now if the extent of

the tropics decreases, the distance of the pole of the heavens from the pole of the ecliptic must decrease, and I don't quite see how the circumference of a circle can decrease its distance from its centre, at least if I know the correct definition of a circle."

I referred this officer to the various published books on astronomy, in which it was asserted that the pole of the heavens traced a circle round the pole of the ecliptic as a centre, whilst it was also asserted that the tropics decreased at the rate of 45″ annually.

The officer's reply was almost sacrilegious as he said: "It seems to me that to put these two statements, which contradict each other, side by side, is all bosh. If the tropics have decreased, then during this decrease the pole of the heavens cannot possibly have traced a circle round the pole of the ecliptic *as a centre*. It may have traced a circle round some other point as a centre, but the pole of the ecliptic cannot be this centre. If it were the centre, how is it that the circumference is now nearer the centre than it was before? I don't profess to be a profound mathematician, but I know enough geometry to be certain that this theory won't hold water."

This remark, coming, as it did, shortly after I had received a letter from a distinguished astronomer, in which I was assured that a conical movement of the earth's axis produced by a second rotation of the earth round a second axis, was identical in all its effects with a conical movement produced by a swaying movement

of the earth, induced me to believe it possible that another problem connected with this change in direction of the earth's axis, was not quite as well known as it ought to be, and I realised the important fact that if I wished to really understand this question, I must undertake its investigation myself, and not submissively follow the authorities, who had made assertions which it was easy to perceive were incorrect.

At this time I had as one of my assistants a captain of the Royal Engineers, who was an excellent mathematician and geometrician, and who gave me a great deal of assistance in the details which it was necessary to work out. Professor Bradley also assisted me in various ways. In spite of these helps, it occupied me five years to work out completely the problems depending on the movement of the earth's axis, which recorded facts appeared to indicate.

During the time that I was completing my investigations, it was made known to me that certain gentlemen, who are always ready to secure an idea and bring it out as their own, were inquiring as to the subject on which I was engaged. Being under the impression that probably my ideas might be pilfered and brought out by these gentlemen, I published a small work termed "The Earth We Inhabit," in which no mention was made of the subject on which I was really engaged, but which was supposed to contain the discovery which I had made. This little book had the effect of putting the would-be pirates off the scent, and I had no further

trouble with them, but was enabled to pursue my investigations without interruption. Had I been at that time as well acquainted as I am now with the peculiarities of the types of mind with which I had to deal, I should have realised the fact, that I need not have been alarmed. Instead of being afraid that some other geometrician would discover my idea, and work it out before me, I should have known that the difficulty was enormous, in inducing the routine mind of astronomers to move out of a groove, or to perceive the accuracy of any novelty which was in opposition to their previous teaching.

Had any person informed me that an individual acquainted with geometry, could assert that the centre of a circle could vary its angular distance from the circumference, and could yet be the centre of this circle, I should have considered the statement too absurd to be believed. I had, however, much to learn on this subject.

The problem, however, which I had now to investigate was as follows: The course said to be traced by the earth's axis on the sphere of the heavens is a circle, having for its centre the pole of the ecliptic. That the earth's axis traces a circle round the pole of the ecliptic *as a centre* is impossible, because the recorded observations of the past two thousand years prove that this centre has gradually decreased its distance from the circumference. As this circle having this centre is impossible according to the well-known properties of a circle,

and its circumference, it remains to be proved what is the curve which the earth's axis produced actually traces on the sphere of the heavens.

The most elaborate investigations were necessary in order to solve this problem. The catalogues of stars framed by Ptolemy 140 A.D., by the Arabians, by Ulugh Beigh, and by Flamstead, had to be examined to find, not only the approximation of this curve, but also whether it was the pole of the ecliptic which had altered its position since the dates of these catalogues. These investigations occupied me several years, and I at length came to the conclusion that the curve traced by the earth's axis was, as before stated, a circle, or so near a circle that it must be either a circle or an ellipse, of which the minor axis was not very different in length from the major axis. But the centre of this circle, instead of being the pole of the ecliptic, as had hitherto been erroneously stated, was a point 6° from the pole of the ecliptic, and having a right ascension of a little more than eighteen hours.

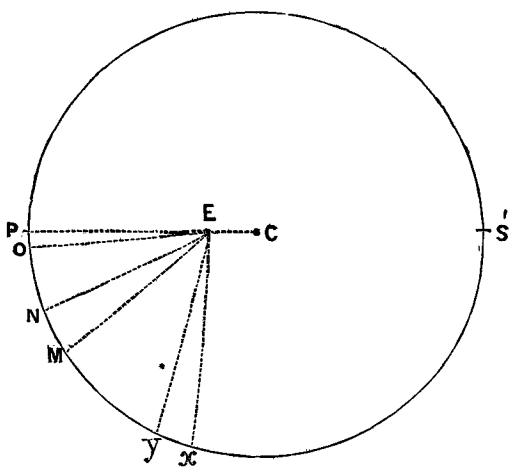
The curve, thus defined, enabled me to frame a geometrical formula, by which I could calculate the exact extent of the tropics, or, as it is termed, of the obliquity of the ecliptic, for any date in the past, and the calculation agreed with minute accuracy with the recorded observations at the various dates. I could show why the decrease in the obliquity took place more slowly now, than it did formerly, an explanation never before given, and as regards this decrease, I could arrive by calculations

at results which never before had been attained except by observation, or an empirical rule doctored by a certain amount of fudging.

The very great accuracy with which this curve agreed with all past observations, proved, that as far as history carried us, the curve answered to the conditions; consequently the course traced by the earth's axis now explains recorded observations, and did not present at its very commencement a geometrical absurdity. The statement hitherto made by the olden astronomers, viz., that the earth's axis traced a circle round the pole of the ecliptic *as a centre*, but that the circumference of this circle had always varied its distance from this centre, seemed, upon calm consideration, to present so singular a contradiction, that, to those unacquainted with the peculiarities of minds which had been crammed, not taught to reason, the continued copying of this statement by various writers on astronomy, must appear like indications of mental imbecility.

The curve which I found answer all the conditions had a radius of 29° 25′ 47″, being made up of 23° 25′ 27″ plus 6°, the 23° 25′ 27″ being the amount of the obliquity when this obliquity became least, and the date at which this obliquity became least was the year 2295 A.D. At that date the annual variation of the obliquity would be nothing; beyond that date the obliquity will increase; previous to that date it will decrease, but the decrease will not be at a uniform rate—the nearer we approach the year 2295 A.D. the less will be the annual decrease.

The reason why this is so will be readily understood by any person who examines the following diagram. The course which the earth's axis really traces relative to the pole E of the ecliptic is shown by the curve xyMP. The angular distance of E from that part of the curve at which the pole of the earth is situated at any given time, is the measure of the obliquity of the ecliptic at that time. For example, when the earth's axis points to



that part of the heavens indicated by x, then x is the pole of the heavens at that time, and the length of the arc Ex is the exact value of the obliquity at that date. The decrease in the distance from E of the curve between x and y is considerable, whereas when the pole is at M the decrease in the length of ME and NE is much less than the decrease in length of xE and yE. When the pole has moved from O to P, the difference in length

between OE and PE is very slight. It is thus evident that the nearer the pole approaches the point P, the less will be the annual decrease in its distance from E, or in other words, the less will be the annual decrease in the obliquity. When the pole is at x, the annual decrease in the obliquity will of course be much greater than when the pole was at M, N, or O.

Knowing that the distance EC was 6°, that EP was 23° 25′ 47″, and from recorded observations knowing that the pole moved along the curve xyMP at the rate of 20″ 158 annually, it was easy to frame a formula which would enable a geometrician to calculate the value of the arc between E and the pole at any date, or, in other words, to calculate the value of the obliquity for any date. This formula is as follows:

$$\{2295—T\}\ 20''\ 158 = A$$

$$\frac{A}{\text{Sine } 29° 25' 47'' = C}$$

$$\text{Cos C. tan } 6° = \text{tan B}$$

$$\text{Cos O} = \text{Cos } 6° \left\{ \frac{\text{Cos } 29° 25' 47'' — B}{\text{Cos B}} \right\}$$

T, is the date for which the obliquity is required; O, will be the value of the obliquity at that date. Having found that by this formula I could obtain results with an accuracy most satisfactory, and that hitherto a mere empirical rule alone enabled astronomers to come somewhat near similar results, I concluded that the curve, which fulfilled such severe conditions, was more likely to

be correct, than the singular curve which had been stated to be a circle which never varied its distance from its centre, and yet did vary its distance from its centre as much as 45″ per century.

Up to this point I had kept merely to that portion of the subject which enabled me to find the value of the obliquity by calculation. No records existed beyond 2,000 years ago to enable me to check calculations by recorded observations, at that date. As far as recorded observations could be found, my curve agreed with these.

On continuing the curve traced by the pole to a point 180° from P and marked S in the diagram, I found that the distance between S (the then position of the pole of the heavens) and E (the then position of the pole of the ecliptic) would be 29° 25′ 47″ + 6° = 35° 25′ 47″ (S C + C E). That is, at the date when the pole was at S, the obliquity of the ecliptic would be 35° 25′ 47″, and the extent of the arctic circle and the tropics would be of a like amount. The pole, if it has moved uniformly, would have been at S at about the date 13,000 B.C.

It was impossible to obtain any human records of astronomical observations at that date; but I believed it possible that the testimony of the rocks themselves might be obtained, because, an extension of the arctic circle from its present position of about 23° 27′ to 35° 25′ would cause all latitudes, north and south down to 41° 35′, to have been within the arctic circle at that

It was therefore most probable that indications would be found of a singular climate having occurred just previous to the present condition of our earth; a climate which was not very different either for the tropics themselves or the arctic circle, but very different for latitudes varying from about 45° to 50°, or what may be termed middle latitudes. The arctic climate which at present extends to latitudes of about 66°, would, when the pole was at S have extended to 54°. Consequently each winter the whole of Scotland, and a portion of England, would have been subjected to an arctic climate, during which time there would have been vast accumulations of snow and ice in these localities. At midsummer each year the sun would have an altitude at midday of 12° more than at present. Consequently, these middle latitudes which had been subjected to an arctic winter, would, six months afterwards, have been subjected to an almost tropical summer. The slow rate at which the pole moved would give about 12,000 years of this variable climate for middle latitudes.

In order to find whether any evidence existed of such conditions having prevailed, it was necessary to look up the researches of geologists, and I found at once in the glacial epoch, exactly those records which I required.

So far every recorded fact tended to prove that the movement I defined was correct, but there was more yet to be demonstrated.

Having found that the true movement of the pole which I have described was not only unknown, but it

was unsuspected by routine astronomers that there was anything incorrect in the accepted movement, I considered it more than probable, that the difference between the true movement and the accepted movement, although very slight, was yet sufficient to account for some of the mysteries which it was known existed in astronomy. On tracing out the true movement, I found that it accounted for the supposed separation of stars near a point in the constellation of Hercules, and that the so-called proper motion of the stars was, in most cases, a delusion. Other mysteries also obtained a clear explanation by this movement of the earth's axis, and, having obtained the careful consideration of Professor Bradley and three or four other able geometricians on my problems, I considered that after ten years of investigation I might venture to make the problem known. The steps I took to do so, and the results, are, I consider, of great interest in various ways, and deserve a special chapter to themselves.

CHAPTER XVI.

Early Training Problems—Practical Advice—Scientific Reception of a Paper—Geologists' Knowledge of a Circle.

In order that the reader may understand the state of affairs at this period of my experiences, I will give some details, and also a summary of the conditions. I must first refer to some matters of a personal nature, and I do so with regret, because reference to these might by some persons be claimed as a proof of an attempt at self-laudation. It is, however, necessary to the clear understanding of the events which will be recorded further on, that these personal matters should be known.

As a boy I had been trained and intended for a civil engineer, and had been accustomed to the use of the theodolite, sextant, and compass, before I was fourteen years of age. I was then prepared for Woolwich, passed all my examinations, and finally ran a dead heat for first place in mathematics at the last examination. My course in mathematics consisted of arithmetic and algebra, trigonometry, conic sections, roofs and arches, statics and dynamics, maxima and minima, hydrostatics, projectiles, and the differential calculus.

During a voyage of seventy-nine days in a sailing ship, which, almost immediately after I had gained my commission conveyed me to the Cape, I learned practically the method of finding the latitude by meridian altitudes, and by altitudes near the meridian. Also how to find the local time by a single altitude, and hence the longitude. I also learned how to take and work out a lunar. There being only two passengers beside myself, time would have hung heavily on hand, had I not have had these amusements.

The captain of the ship, who was part owner, was a very superior man, and understood thoroughly the subjects that we worked out together.

Shortly after being appointed to the Royal Military Academy as Instructor, I obtained permission to attend at Greenwich Observatory, and to there learn the detail working of the various instruments in use at that establishment, and the "reduction" of the observations.

Three mornings per week I could attend for practice in these matters, and any night that was clear enough for observations I attended, and used my transit instrument for finding longitude by meridian transits, and latitude by observations in the prime vertical.

During upwards of two years I attended at the Observatory, and went through the detail work of the computers, and learned all that could be taught. Whilst I acquired a knowledge of all that was known at the Observatory, I gained also certain information which was

afterwards of great value, for I found out what was not known, and not even suspected.

Having had the advantage of this preliminary training in connection with astronomy, I then had the experience of teaching (not boys) but well-informed men, the various problems in astronomy, and I had to meet every objection raised by these students, against what they supposed to be errors, or defects, in the subjects I taught.

When I compare the training through which I passed to qualify me for judging in problems in astronomy, with the training which certain other gentlemen, who claim to be authorities, have received, I am indisposed to admit that the mere possession of a large telescope and an observatory, or a magic lantern and a synopsis of other men's discoveries or opinions, entitles such individuals to claim, that their mere opinions are infallible, and that the laws of geometry are to be ignored, if these laws contradict their theories. It may be, that these individuals are gifted with a profound knowledge; but being a reasoner, I am indisposed to grant this, unless some proof is given me, other than an assumption of wisdom, of which there is a total absence of evidence.

I deem it necessary that the above fact should be made known, for reasons which will be manifest to the reader as he proceeds in this history.

The problems for my consideration were now as follows:

1st.—The astronomers of all nations believed that the earth's axis traced a circle round the pole of the ecliptic as a centre.

2nd.—They knew from the recorded observations of many centuries, that the obliquity of the ecliptic had been gradually decreasing. But they did not know whether the *rate* of this decrease was an increasing or a decreasing rate.

3rd.—Although they knew that the obliquity of the ecliptic had so decreased, they seemed to have entirely overlooked the facts, that as long as this decrease occurred, it was impossible that the pole of the heavens could trace a circle round the pole of the ecliptic *as a centre.*

4th.—Although the cause of the variation in the obliquity of the ecliptic was not known, or the actual course traced by the earth's axis, yet a theory was invented by certain theorists, who asserted that the decrease in the obliquity would never amount to more than 1°.21′.

5th.—In consequence of the theory named in No. 4, it was affirmed that astronomy was totally unable to give any explanation to geologists of the cause for the marvellous changes of climate proved to have taken place on earth, and that the fact of an arctic climate having prevailed down to 54° latitude shortly before the present conditions on earth, was a question with which astronomy had nothing to do, as astronomy could give no explanation of such a change.

This was the fortification which stood before me, and that which I had to bring against it was the following:

1st.—The assertion that the pole of the heavens traced a circle round the pole of the ecliptic as a centre, was a theory invented when it was supposed that no variation in the obliquity of the ecliptic occurred.

2nd.—When it was ascertained beyond a doubt, that a variation in the obliquity of the ecliptic did occur, it escaped the notice of the astronomers at that date, that the theory of the pole of the ecliptic being the centre of this circle, was a geometrical impossibility.

3rd.—It having been overlooked that the pole of the ecliptic could not be the centre of this circle, it became a dogma of astronomy that the pole of the ecliptic was the centre, and theory after theory was therefore built on this erroneous belief, and it came to be considered an axiom, that only some trifling variation could occur in the obliquity, and this variation could not be due to any movement of the earth itself. Consequently, in spite of the array of geological facts, which tended to prove that an arctic climate extended down to 54° latitude, shortly before the conditions now prevailing on earth, yet these facts were either ignored, or considered beyond the explanation of astronomy, and it was considered more in accordance with exact science to affirm that the

centre of a circle could vary its distance from its circumference, and yet always remain the exact centre of this same circle, than to admit that the curve traced by the earth's axis, although still a circle, had another point for its centre removed 6° from the pole of the ecliptic.

4th.—The course traced by the earth's axis, which I had defined, explained every fact which the erroneous theory then accepted explained, but in addition my movement explained the decrease in the value of the obliquity, it explained why the rate of this decrease was a decreasing rate, it explained the so-called proper motion of the fixed stars, and it explains the cause of the glacial epoch of geology.

It also enabled me to obtain, by calculation, results which could not previously be obtained, except by an empirical rule, or by fudging, and then only with approximate accuracy. It, therefore, seemed to me that I had but to whisper the key of the problem, in order to have the whole question viewed and accepted at once.

I soon discovered that I had yet much to learn connected with the peculiarities of the human mind, and also with the manner in which new truths were received by men, who were considered scientific, and whose reputation was in danger of losing some of its importance.

My first step was now to consult some friends on whose judgment I could rely, as to the course I should adopt, to make known, and to bring into notice the problem on which I had devoted so many years of

investigation. One of these friends (the late William Howitt) was a man of great experience in the literary world, and after he had understood my problem, gave me his opinion in the following words:

"I can see that you have made a discovery in astronomy which is an entire novelty. It follows, therefore, that those who accept the present theories are wrong. If your problem is acknowledged those astronomers, who have accepted, and taught, the present accepted theory, will be proved to have been wrong. Do you expect these gentlemen to help you to prove they have been wrong, or do you anticipate that even granting they are capable of understanding your problem, they will do their best to aid, or to oppose you? By the aid of your problem, results can be simply obtained by calculation which now require dozens of men, termed computers, to observe and register. These men all derive incomes from their work. Do you expect these men to help bring your problem into notice, when the doing so will take the bread out of their mouths? Again, a number of gentlemen have received the gold medal of the Royal Astronomical Society for their theories or accumulated observations, to account for that which you clearly demonstrate does not occur. Will any of these help you to bring this problem into notice, or will they try to condemn it?

"There is a special peculiarity about astronomical novelties of the class to which yours belongs. Bigoted scientists asserted that a railway engine could not travel

at twenty miles an hour. But Stevenson made an engine which did travel at that rate, and consequently proved by a fact, that of which he could not convince these opponents by reason. Another so-called philosopher asserted that he would eat the first steamer, coals and all, that crossed the Atlantic by the aid of steam alone; a steamer was built and did cross the Atlantic—so further argument was useless. In both these cases the accuracy of the reasoning was proved by a fact. Now, if you had predicted that some astronomical event, visible to every person, would occur at some given date, and this event did occur, you would have a much easier nut to crack. In your case, you have to appeal to reason, and you show, that the assertion of the pole of the earth moving in a circle round a point, said to be its centre, and yet varying its distance from this centre, is impossible. But this impossibility is not perceived by minds ignorant of geometry, nor will there appear to these any flat contradiction in the assertion, that, although the obliquity of the ecliptic has continued to decrease during the past 2,000 years, yet the pole of the heavens during all that time has moved in a circle round the pole of the ecliptic as a centre. You have already informed me, that a high authority on astronomy stated that no matter whether the conical movement of the earth's axis was caused by a swaying movement of the earth, or by a slow rotation round a second axis, the effects on all parts of the earth would be identical. Any man who made this statement must have possessed a mind so thoroughly blind to

common sense, that his reasoning on any subject would almost certainly be erroneous.

"The majority of men will not reason on your problem at all; they will merely look at what previous authorities have stated, and when they find that you give a different cause for a known effect, they will not weigh the relative value of the two explanations, but will cling to that which has been taught them. A far more powerful opposition, however, will be given by those whose interests are likely to be affected, if it is admitted that your problem is true, whilst that which they have advocated is false. What, then, becomes of the reputation for profound knowledge which these individuals claim for themselves? When these persons have asserted that the facts of geology cannot possibly receive any explanation from astronomy, what opinion would be formed of their knowledge, when it is proved that the same movement of the earth's axis which explains the decrease of the obliquity, the changes in polar distance of the stars, and the precession of the equinoxes, also explains the cause and gives the date of the glacial period on earth. Do you expect that you will receive aid from these men, or can you not perceive that every possible means will be adopted to keep you out of court? The general public are, as a rule, ignorant of the mere elements of astronomy, and none of these would dare offer an opinion on what they would consider so very abstruse a problem. Ask any one whether the centre of a circle was always at the same distance from

the circumference, and he would laugh at so simple a question being asked him. He would reply, of course it must always be at the same distance. If, however, you said the pole of the heavens traces a circle round the pole of the ecliptic as a centre, but as the obliquity of the ecliptic varies, this centre must vary its distance from the circumference, he would probably reply, no doubt it must; but I know nothing of astronomy, and, therefore, cannot give any opinion. You may, and probably will, obtain some reasoners capable of impartially judging the accuracy of your proofs; but it will be ten or twenty years, or probably more, before it attracts any attention."

These remarks were not very encouraging, but did not much surprise me. The experiences which I had gained when I first endeavoured to bring range-finding into notice, had taught me that obstruction seemed the normal condition of some minds; I therefore decided that I would endeavour, by two proceedings, to make the problem known, on which I had devoted so many years of investigation.

The course which I decided to adopt, was the following.

To prepare a paper, and read it before the Royal Astronomical Society, of which I was a Fellow, and challenge a discussion. To prepare a second paper and have this read before the Geological Society, and invite a discussion. To publish the problem in two volumes, one dealing with the geological, the second

with the astronomical portions of the subject; and to have these two volumes ready for the press shortly after my papers were laid before the societies named.

The evening at length arrived when I was to read my paper before the Royal Astronomical Society, and when I looked forward with considerable interest to the discussion which I anticipated would follow. There was no part of my problem which I was not prepared to defend; while the demonstrations had been worked out with the greatest geometrical accuracy.

I read my papers, and waited for the discussion. After a lengthy silence, one distinguished astronomer remarked, that he felt quite unable to offer any remarks, as the subject was so new, that he had never considered it. Another gentleman inquired whether my paper was not opposed to the accepted theories of the day. The final and crushing argument, however, came from a venerable member of the Society, who announced, that on looking in Herschel's "Outlines of Astronomy," he found it there stated, that the pole of the heavens traced a circle round the pole of the ecliptic as a centre, maintaining a uniform distance of 23° 28′, and never varying its distance. Also, that it was a well-known fact that the obliquity of the ecliptic did vary, but that the variation in the obliquity in no way depended on the distance of the pole of the heavens from the pole of the ecliptic, as these two poles never varied their distance!

This profound assertion terminated the discussion, and the next paper was eagerly demanded. This next

paper was one relative to a squabble between two young computers, one of which thought a lunar crater when he looked at it was active, the other thought it was not so. A most vigorous discussion ensued on this important question, which was not concluded when I left the meeting.

Although no advance appeared to be made in making known my problem, I trusted that when the papers were given in "the records," it would draw the attention of those who had not been present at the meeting. On receiving the notice of my paper, I found only a few lines given to it, but in those few lines my meaning was entirely perverted, whilst an almost verbatim report was given to the computers' squabble.

There was yet another tribunal at which I anticipated a different result, viz., at the Geological Society. Geologists had continued to assert that astronomy (if correct) must be able to account for the great climatic changes which had occurred on earth; my problem did account for these changes, and I therefore anticipated a more favourable reception. My paper was read, and it was admitted that the facts of geology were explained by the problem which I there demonstrated. But, said the geologists, it was an astronomical question, and they must wait for the verdict of astronomers. They, as geologists, did not feel qualified or competent to say whether the centre of a circle could vary its distance from the circumference, and still remain always the centre of the same circle. They did not know enough

about a circle to be able to offer any opinion. The problem was one belonging exclusively to astronomy, and though my demonstration explained the facts of the glacial epoch, particularly why these facts were visible in centre positions, that is, in middle latitudes of each hemisphere, yet they did not like to accept the explanation, in consequence of its being a question entirely for astronomers, whether the centre of a circle might not in astronomy vary its distance from the circumference, and yet be always the true centre of the same circle.

The charming simplicity of such arguments, showed a trusting faith, which in a good cause is most commendable. Geologists, whose researches had been recorded with admiration, announced that they did not feel qualified to judge whether the centre of a circle could vary its distance from the circumference. In Euclid it is stated it cannot do so; but then Euclid never mentioned an astronomical circle, so perhaps an astronomical centre can do so; whether it could or could not, geologists did not feel competent to offer an opinion, and so this trial failed to produce much effect.

From among this crowd, one geologist singled himself out. He had examined my problem, and though previously unknown to him, he at once wrote to me, saying that he had carefully examined my problem, and was convinced it was the only sound explanation of the facts of the glacial epoch. This gentleman was Professor Belt, the author of the "Naturalist of

Nicaragua." He encouraged me to proceed, and gave me some valuable information relative to what he had seen in connection with boulders, etc., in various parts of the world. Some time afterwards he wrote papers in the *Quarterly Journal of Science* in which he boldly stated, that my problem alone explained the known facts.

In the meantime I had published my two volumes: "The Cause, Date, and Duration of the last Glacial Epoch," and "The Proper Motion of the Fixed Stars." In these two works I gave the details of the problem and the results to which these led. These works having been before the public upwards of eleven years, I will now venture to refer to the adventures they have experienced.

CHAPTER XVII.

Incompetent Critics—A Circle Impossible, yet Accepted—Curious Belief about the Sun.

At the commencement of the year 1873, I published the first of the two works named in the last chapter. In 1874 I published the second work.*

I was fully aware that so great a novelty as that which was brought into notice in those volumes, would be attacked in every possible way. It was my wish that it should be attacked, for I was convinced that any problem, or theory, which could not stand against a *sound* objection, was worthless. It was this conviction which caused me to conclude, that the accepted theory of the pole of the heavens tracing a circle round a point as a centre, from which centre it gradually varied its distance, was an erroneous theory. The fact of such a geometrical impossibility having been accepted as correct, during even the hundred and more years that it had been accepted, did not disprove the impossibility of the theory. That which it did prove was, that the impossibility of the two statements had never attracted the attention of a competent geometrician.

* Chapman & Hall, publishers.

To examine, and consider the objections that would be urged against my problem, was a proceeding to which I looked forward with considerable interest. I had tried hard to find a flaw in the problem, and had failed to do so. I had asked friends who were mathematicians, or geometricians, to attempt the same criticism, and though several objections were brought, yet the suggestors of these admitted that their objections were unsound, as soon as I pointed out that these objections themselves were incorrect. That some outsider might suggest some novelty as an argument against the problem was possible, but I did not think it was probable; so that whilst I waited patiently for these criticisms, I also looked forward to them with considerable interest, because, from the experience I had already gained, I believed that the mental condition of some of our leading scientific authorities was not as healthy, or as sound, as it was generally assumed to be, and by means of the arguments, or objections they used against my problem, I could form an opinion of the effects of the cramming, or long course of routine work, through which these gentlemen had passed.

One of the first notices of my book (in which book I pointed out that the earth's axis could not trace a circle round the pole of the ecliptic *as a centre*), stated that the object of my book was *to prove that the earth was not round.* The writer then proceeded to express his opinion, that so absurd a book was not likely to be published by any officer who entered the army in the present day,

as *some* education was now given to officers, whereas when I (the Author) entered the army, no education was considered necessary.

It seemed curious how mistaken a person may be about himself. I was under the impression that I had been taught some mathematics, and geometry, before I entered the army, and that the reader may perceive that I had some grounds for this belief, I refer him to the course of mathematics described on a recent page, in which course I gained the highest place at the examination. These are my facts; but an anonymous writer knows better than I do, and assures his readers that I never learnt mathematics at all. But the most singular part of the assertion is, that my book was written for the purpose of proving the earth flat, and not round. Now probably some people who knew what was in the book, and who read this assertion of the critic, would conclude that such a statement was a malicious falsehood, circulated by an unprincipled person, for the sole purpose of endeavouring to prevent people from reading the book, and also to cast ridicule on the author. I do not attribute either proceeding to such a critic, I believe he wrote conscientiously and to the best of his ability. He probably read the sentence "earth's axis does not move round pole of ecliptic as centre," and certain words in this sentence were comprehended, others misunderstood; those which remained were, "earth's, not round," and straightway he wrote his review, and thought he was doing a very clever thing. I do not believe that the

gentleman who asserted that no steamship could ever cross the Atlantic, made the statement from malicious motives; he merely asserted what he believed, and fancied he knew. The defect lay, not in his want of honesty of purpose, but from the fact of his mind being entirely impotent, and incompetent to reason.

Another critic pitied my ignorance of the elementary laws of astronomy, for, said this writer, had I been acquainted with these laws, I should have known that all *the celestial bodies* move in ellipses, not in circles, so when I say the *earth's axis* traces a circle round a point 6° from the pole of the ecliptic, I am asserting that which is known to be impossible, and he for one would adhere to the present accepted theory.

When I read this profound objection, it is surprising that I did not at once consign my books to the flames. I happened to remember, however, that the present accepted theory which this person elected to accept, was that the earth's axis *traced a circle* round the pole of the ecliptic as a centre, and not only traced this circle round this centre, but accomplished the wonderful performance of varying its distance from its centre. I also realised the fact that the zenith of each locality on earth traced a circle in the heavens during each rotation of the earth; and I had observed this fact so often, that I was bound to believe it, in spite of the assertion of this learned writer, that no such thing ever could occur, because *the celestial bodies* moved in ellipses, not in circles.

Having, as he no doubt believed, demolished my problem, by asserting that it could not be true, because the earth's axis could not possibly trace a circle, he seemed immediately to forget what he had written, and said, that he for one will accept the theory that the earth's axis does trace a circle.

We have here again an example, not of maliciousness, but merely an exhibition of feebleness of the reasoning powers, and probably a little too much self-sufficiency, causing a person to undertake to criticise a work for which he is mentally incompetent.

The objection raised, viz., that my problem could not be correct, because I stated that a circle was the course traced by the earth's axis, was evidently considered a trump card by those persons who were desirous that no reception should be given to my facts, for this argument was copied and re-copied with additions.

No celestial body moves in a circle. Consequently, said the learned critics, how is it possible that this one movement should be in a circle? Consequently, we must reject this visionary idea, and must adhere (some writers so far forgot their lessons that they added) to the circle which has been hitherto accepted as correct.

Other writers at once attempted the pompous style, and asserted that they had hoped the days were past, when men would attempt to prove any problem by means of mere geometry. What, they urged, has geometry to do with astronomy? And how ignorant a man must be, who imagines that he can by geometry

prove anything. What was to take the place of geometry, these profound gentlemen failed to explain; but, as they put forward their own opinions as infallible, it led one to believe that if they asserted that two sides of a triangle were less than the third side, or that the centre of a circle was not always at the same distance from the circumference, then when geometry demonstrated them to be in error in their opinions, it was so much the worse for geometry.

Occasionally there would be evidence of some writer's having learnt the elements of the problem, and to have discovered that it might not deceive every person to assert, that because a circle was the curve defined by me, therefore the problem could not be true. Probably these objectors had possessed sufficient intelligence to discover, that the orthodox theory was that the pole of the heavens did trace a circle. So to assert that my problem could not be correct, because I defined what the circle really was, would have failed to pervert my evidence. Another objection must therefore be raised, and many amusing arguments were used, the most interesting of which is perhaps the following, given by a gentleman in a book, in which he assumes to give the relative value of the various explanations of the glacial epoch.

This writer states, Colonel Drayson is quite wrong in supposing that the midday altitude of the sun has anything to do with the heat or climate of any locality. The equatorial regions of the earth are hot, *because* they

are situated at the equator, and the arctic regions are cold, *because* they are more than 66° from the equator. Now if the obliquity of the ecliptic varied, so that even Scotland were brought within the arctic circle, there would not be the slightest difference in the climate of Scotland; and why? because if Scotland were within the arctic circle, and the sun never rose above the horizon during many days and nights, it would have the same climate as at present, *because* it would still be at the same distance from the equator, and it is the distance of places from the equator which affects their climate, not the midday altitude of the sun, or the length of time it remains above the horizon.*

This last argument is, I consider, a more valuable record of the incapacity for reasoning possessed by certain minds, than either of the former examples. This last writer does away with the sun altogether, and seems to consider it is not necessary. If the sun ceased to give light and heat, localities on earth would not have their distance from the equator altered one atom, and, therefore, according to this previous opinion, all climates would remain the same as at present. They might so remain, but I don't think they would.

Arguments and objections of a similar class, were urged against my problem; some opponents attempted to swagger, others to hiss, several confined themselves to

*England during December is at the same distance from the equator that it is in June. I have found, however, that it is usually colder in December than in June.

personal abuse, or sneers, but what I anxiously looked for, and failed to find, was one single sound or logical objection. All the sneering or abuse of ignorant men will never shake my conviction that the three angles of a plane triangle amount to 180° exactly, or that the centre of a circle can never vary its distance from the circumference; when, then, I find men of great reputation for scientific knowledge asserting the opposite of these two geometrical truths, I can only come to one conclusion, viz., that their reputation is undeserved.

Twelve years have elapsed since my book was published, containing the solution of the mystery as regards the cause of the last glacial epoch, and giving to this epoch at its height, the date about 13,000 B.C. During this twelve years not one single proof, argument, or evidence, has been brought forward against my problem, which was not of a puerile, erroneous, or feeble kind. The gravest misrepresentations have been made as to what the problem really was, but these I attribute to the ignorance and incapacity of the persons who invented these; and the conclusion which I have arrived at is, that the minds of those persons who ought to receive such a problem are not in a sufficiently receptive state to perceive what the problem really is, they having been so incapacitated for reasoning, in consequence of a long and continued course of cramming certain theories, which theories are based on incorrect data.

CHAPTER XVIII.

Results of Cramming—Change in Direction of the Earth's Axis—Tests of Knowledge—Ancient and Modern Arguments—Examination Paper—Problems for Solution—Facts *versus* Opinions.

In the earlier pages of this book I mentioned that what is termed education ought to consist of two special branches: first, to store the mind with certain well-ascertained facts, which could be made use of in after life, by the aid of memory alone; secondly, to accustom the mind to reason, and to so cultivate this faculty that men, whose minds had been thus trained, should be able to use their reason, when the novelties, which must occur to them in after life, have to be dealt with. Men whose minds have merely been stored are, in the majority of cases, the obstacles to all progress. They too often obtain official positions of importance, and are expected to sit as judges, on various questions of importance. If their minds have been stored, and not trained to reason, they will to a certainty come to a wrong conclusion. They will not reason on the facts presented to them, and then give a decision, but they will call to mind their stock knowledge, and give decisions according to their preconceived opinions. If they venture to offer any arguments

or reasons for their decision, these will, to a certainty, be so absurd, as to cause even an uneducated boy to smile. That which occurs in small matters, is repeated in large; and as an example of such types of mind I give the following as an instance, which has many times been acted in my presence. A gentleman playing whist held the thirteenth trump, and knew it was the thirteenth. His partner led him a winning card, which he knew was a winning card; and this winning card he trumped with his last trump. On being asked why he trumped his partner's trick, he replied: "Because I was anxious to make my last trump." "But," said his partner, "your last trump must make; there was no possibility of your not making it." "Well," replied the gentleman, "I wanted to make sure, so I trumped your trick." If, now, we attempt to realise the class of mind possessed by such a man, we shall find it excessively difficult, yet such classes of mind are common. I have met hundreds such. This person may have played whist during thirty years, and will yet commit such acts. Had he been trained as an astronomer, the fact of his having learnt astronomy would not prevent him from giving similar arguments on astronomical questions. He is incapable of reasoning, and though he may possess a very large amount of stock knowledge, yet when a novelty is presented to him, he has not the reasoning capacity to bring to bear on it. He is, in fact, a most dangerous man. If he had been in the army, and by the mere force of seniority had risen to the rank suffi-

cient to gain him a command, the same incapacity to reason would have been exhibited, as soon as the conditions were not those. where his stock knowledge could be applied. Set him in opposition to an enemy, whose method of warfare was not in accordance with the text books with which he had been crammed, and a disaster would be as certain to follow, as that he would play a wrong card at whist.

A man who has been crammed on one subject may be capable of forming opinions on this subject, but only as far as he has been crammed, and only in cases where his stock knowledge can be applied. Put before him some other subject, or a novelty, and he is of all men the least capable of judging these, unless he is a reasoner. If, however, a man has been trained to reason, he can form a very fair opinion on any subject, however new to him, when the facts are placed before him. Reasoning is for all time, and all subjects; cramming stock knowledge holds good only for the one subject which has been crammed, and what is the most dangerous to progress, it holds good for only so far as this special subject may at the time have advanced. The astronomers at the time of Galileo had been crammed with the theory of cycles and epicycles of Ptolemy and his school, and were thoroughly acquainted with these. To them the first law of astronomy was that the earth was stationary, and that the sun revolved daily round the earth. This was stock knowledge crammed into their minds, but not

reasoned upon, just as stock knowledge is crammed at the present day. When, then, the probability of the earth rotating on its axis, was a question for these men to reason on, they were as unable to do so, as any right-handed writer would be to at once write with his left hand. These men could not perceive the absurdity of the objections they brought forward against the theory of the earth's rotation. To them, the most ridiculous belief that could be accepted was, that the earth rotated; whilst the most exact science was, that it was stationary. A reasoner, however, might probably have argued as follows. Every planet that we see is round, *i.e.* spherical. The earth is spherical. Every planet, as also the sun, rotates on its axis. Is it not reasonable to suppose, that the earth rotates on its axis also? Then, again, assume that the earth does rotate on its axis, will this rotation explain the apparent rising and setting of the celestial bodies? It would be found that the earth's rotation would explain all these well-known facts. Where, then, is the absurdity of the theory that the earth does rotate?

Let us now refer to the problem which I have brought forward.

Observation has proved that the earth's axis changes its direction at the rate of about 20″ annually; and when it was not known that there was a decrease in the obliquity of the ecliptic occurring, it followed that the pole of the heavens must maintain a uniform distance from the pole of the ecliptic, because the angular distance

of these two poles is the same as the angle formed by the ecliptic and the equinoctial. It followed then, as a geometrical law, that *if there was no change* in the obliquity of the ecliptic, whilst at the same time the pole of the heavens moved 20″ annually, the course which the earth's axis traced on the sphere of the heavens must be a circle, the centre of which circle was the pole of the ecliptic, from which centre there was no variation in distance. When, however, it became known that there was an annual change, viz., a decrease in the value of the obliquity, and that this decrease had been going on since the earliest date of which we had any record, then, of course, the veriest tyro in geometry should have known, that the distance of the pole of the heavens from its assumed centre must have decreased to a like amount; and, consequently, the curve traced by the pole of the heavens could not be a circle, having for its centre a point, from which it varied its distance. The course traced by the pole might be a circle, having some other point for its centre; but that which was an impossibility was, that the pole of the ecliptic could be this centre.

No stock knowledge, other than an acquaintance with the properties of a circle, is requisite to enable a reasoner to feel confident of this fact; and we can now advance to the next inquiry, viz., what do the observations of the past thousand years prove, as regards the nature of the curve which the earth's axis does trace? These observations prove that the curve is, as before stated by earlier astronomers, a circle, but the

centre of this circle is not the pole of the ecliptic, but a point 6° from it. Consequently, although the pole of the heavens decreases its distance at the present date from the pole of the ecliptic at the rate of about 45″ per century only, it in former times decreased at a much greater rate, as will be seen by looking back at the diagram on page 206 of this work. This curve and course traced by the pole, being carried back to 13,000 B.C., gives the distance between the two poles, 35° 25′ 47″. The annual variation of climate, therefore, at that date, would be very different for middle latitudes from that which now prevails. England, and Scotland, would in winter be subjected to an arctic climate. Vast icebergs would be formed in the north of England, capable of transplanting heavy boulders to distant localities. Scotland, and Wales, would have their glaciers and moraines, the records of which are now scratched on the rocks. All these facts are now accounted for, not by a guess or a theory, but by merely tracing out the curve which the earth's axis has traced on the sphere of the heavens, during the past 1,000 years. Are we still to go on asserting the dogma, that astronomy can give no explanation of the facts of geology because the course traced by the earth's axis is a circle round a centre, from which centre the circumference continually decreases its distance, and such a wonderful circle is considered highly satisfactory?

Those who have had much experience in instruction

have probably found, as I have, that there are certain test questions, or problems, by which they can judge of the mental capacity of their pupils, or of the knowledge they possess on a certain subject. As an example, I give the following case:

At midday, the sun has an altitude above the horizon of 50°. Twenty minutes afterwards the sun has decreased this altitude by 10′. Could you from this data inform me in how many hours the sun will set?

If an individual stated that, as the sun decreased its altitude 10′, that is $\frac{1}{6}$ of a degree in twenty minutes, therefore it would take two hours to decrease 1°, and 100 hours to reach the horizon, I should at once know that this pupil was utterly ignorant of the curve which the sun appears to trace from midday to sunset. I certainly should not respect the opinion of this pupil, as regards the movements of the sun, nor should I consider him an authority on such problems.

Exactly similar remarks, however, have been made by certain individuals relative to the rate of the decrease in the obliquity. It has been asserted that the decrease in the obliquity is now 45″ per century. *Therefore*, say these individuals, it will occupy 8,000 years to amount to 1°, and 80,000 years to reach 10°. When I perceive remarks of this kind put forward as scientific facts, I know that the authors of such assertions know no more about the true movement of the earth's axis than would the pupil, who told me that the sun would not set until 100 hours have passed, because when near the meridian

it decreased its altitude only $\frac{1}{6}$ of a degree in twenty minutes of time.

It is a general but erroneous belief that the mental capacity of the ancient men of science was far inferior to that of the moderns; but there is no evidence of this being the case, the stock knowledge of the ancients was less, but their reasoning seems to have been very similar. In fact, given the same conditions of cramming, without cultivating the reasoning faculties, and exactly similar results occur.

Among the ancients, Herodotus was a great traveller and observer, and, for his time, an able man. When a novelty is presented to him, however, he acts in exactly a similar manner to the modern man of science, who has been merely crammed. Writing on the novel assertion in his day relative to the earth being spherical, Herodotus states, "And here I cannot refrain my laughter, when I hear men speaking of the earth as round, as though made in a machine, and that Asia is as large as Europe."

Having expressed this opinion, he commences to swagger, and announces that he will inform the ignorant of the relative size of Asia and Europe: of course his information on this point is all wrong.

When I announced that the pole of the heavens could not trace a circle round a point as a centre, from which centre it varied its distance, and demonstrated that the curve which the axis did trace was still a circle, but round a different point, it was asserted that I could not be correct, because as no celestial body moved in a circle,

therefore a circle could not be traced by the earth's axis as I asserted. Then the persons who have stated this, inform their readers that the true and accepted course which the earth's axis does trace is a circle, round the pole of the ecliptic as a centre.

Of the two statements—that of Herodotus or that of the modern writer—it seems to me, that though both writers exhibit great ignorance, Herodotus shows less actual stupidity. Herodotus again seemed to be aware that the sun had something to do with the climate on earth. But a modern writer practically does away with the sun, and asserts, that no matter what the extent of the tropics, or arctic circle on earth, yet this extent would in no way affect the climate, because localities in the arctic circle are not cold, because the sun never rises during many weeks above the horizon of these places, but because they are more than 66° from the equator.

When it was announced that Jupiter possessed satellites, the crammed men at that date asserted, that such could not be the case, because as it was known that the universe was constructed on the same principle as man, and as man had seven openings in his head, so there could be only seven planets in the heavens; *therefore,* Jupiter could not possess satellites, for if he did, the whole course of Nature would be upset.

Now, by the whole course of Nature, this writer meant the theories in which he and his school believed.

The above statement will seem absurd and contemptible to any modern and educated man; but is

it more absurd than to urge, that the pole of the heavens cannot trace a circle, which a thousand years of observation prove it has traced, because theorists have asserted that it traces a circle round a centre, and yet varies its distance from that centre; or to assert that the curve I have defined cannot occur, for if it did it would interfere with the accepted theories, with which men had been crammed?

The human mind, as far as its reasoning powers are concerned, has not improved since the days of Herodotus. There is the same system of cramming, the same fossilised condition produced thereby, and the same inability to reason on a novelty. And I maintain that these results are mainly due to the fact, that we even now teach and cram, but rarely educate, and consequently we store, but do not strengthen the mind.

In an earlier portion of this work, I stated that I could set an examination paper in astronomy, that not ten in one hundred of the astronomers of any country could answer. These are not questions which cannot be answered, but each can be replied to by any person acquainted with astronomy, so far as the earth's *true* movements are concerned, and with geometry. Here are the questions, and it may be tested who can answer them, without following the instructions given in my work published in 1874, and entitled the "Cause of the So-called Proper Motion of the Fixed Stars."

No. 1.—An observer A, perceives a comet, the nucleus of which is at his zenith, the tail of this comet is visible

60° from the zenith, and appears to this observer as a straight line. An observer B, at another locality on the earth's surface, sees at the same instant, the same comet, the nucleus of this comet is on the meridian of B, and the tail is curved nearly as much as is a rainbow. State where A, and B, are situated, and the reason why the comet's tail appears straight to A, but curved to B.

No. 2.—The stars α and β, Ursæ Majoris, are termed the pointers. Do these stars always appear to an observer in middle latitudes, to point with equal directness towards the pole star? Give a reason why they do, or do not, always point with equal accuracy towards the pole star.

No. 3.—It has been asserted during the past century that the pole of the heavens (that is, the earth's axis produced to the heavens) traces a circle round the pole of the ecliptic as a centre. It has also been asserted that the obliquity of the ecliptic, has slowly decreased during the past 2,000 years. Define first the connection between the obliquity of the ecliptic, and the angular distance of the pole of the heavens, from the pole of the ecliptic. Secondly, explain how it is, that the pole of the heavens traces a circle round the pole of the ecliptic as a centre, when the angular distance of these two poles has decreased during the past 2,000 years.

No. 4.—At the date 1590 the obliquity of the ecliptic was found to be 23° 29′ 54″. In 1690 23° 28′ 48″, equal to a decrease of 66″ for that century, or 0″·66 per year.

In 1840 the obliquity was found to be 23° 27′ 36″·5, and in 1860 to be 23° 27′ 27″, giving a decrease of 9″·5 for 20 years, or only 47″·5 for a century. Show by a diagram why the rate has decreased since 1690, and state, with proof, what the obliquity will be 2,000 years hence, and what it was 500 years ago.

No. 5.—Without making use of the value of the obliquity found for any date by observation, give a formula, based on the geometrical curve traced by the earth's axis, which will give the value of the obliquity of the ecliptic for any date in the past, or future; and show from recorded observations, that the results arrived at by this formula agree with recorded observations.

No. 6.—The gold medal of the Royal Astronomical Society was given some years ago for a paper, on the supposed proper motion of the fixed stars. In this paper it was stated that μ' the proper motion of a star, could be obtained by the following formula:

$$\mu' = \frac{\text{P}-\text{B}}{45} - \frac{p+\pi}{2}$$

Where P denotes the right ascension of a star in Piazzi's catalogue, B the right ascension of the same star in Bradley's catalogue, p the annual precession in 1800, π the annual precession in 1755. Show why this formula is entirely erroneous. Why the results obtained from it are worthless, in consequence of the formula being geometrically untrue, and explain with a diagram why it is so.

No. 7.—Three stars, x, y, and z, have at a given date nearly the same north polar distance, which north polar distance is more than 10°. These three stars are distant from each other 3° in angular distance measured on the sphere of the heavens, so that the arc $xy = 3°$, the arc $yz = 3°$. During three years' observation, it is found that the star x has decreased its right ascension, but the rate of this decrease is an increasing rate, the star y has not perceptibly varied its right ascension; the star z has increased its right ascension, and the rate of this increase is found to be an increasing rate. State in what part of the heavens these stars must be situated, in order that these variations in the rate of the change, in right ascension must occur. Prove by a geometrical diagram that your statement is correct.

No. 8.—Two stars, O and P, are distant more than 40° from the north pole of the heavens, and have each the same north polar distance. Both these stars increase their right ascension. The annual rate of the increase of right ascension of O is an increasing rate; the annual rate of P is a decreasing rate. Show where these stars must be situated to fulfil these conditions.

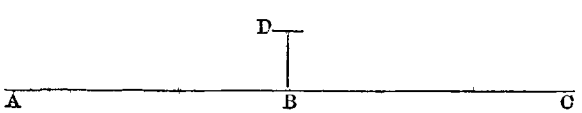
No. 9.—A star, A, has a north polar distance of 12°; a star, B, has a north polar distance of 30°; the two stars differ more than four hours in right ascension. Both stars increase their right ascension uniformly, so that the *relative* right ascension of the two stars never varies. State in what part of the heavens these stars are

situated, and explain the law which causes the relative right ascension of these two stars to be constant.

No. 10.—Referring to the last question, a star, C, has the same right ascension as the star, A, but is only 6° from the pole of the heavens. Will the annual rate in the change of right ascension of the stars, A and C, be the same? State why.

No. 11.—It has been stated that the earth's axis traces each year a portion of a cone, the change in direction of this axis amounting to about 20″ annually. Show by a diagram or by a globe, what changes the other parts of the earth have made and are due to this conical movement only, during the past 1,800 years.

No. 12.—Three points, A, B, C, are on the horizon as shown in diagram; a point, D, is 5° vertically removed from B. The angular distance, AB, measured with a



sextant, is 30°; the angular distance, BC, measured with the sextant, is also 30°; and the angular distance, AC, measured with the sextant, is 60°. Will the angular distances, CD + DA, measured with the sextant, be greater, equal, or less than 60°?

No. 13.—Three stars, A, B, C, are at the same altitude above the horizon as shown in the preceding diagram, this altitude being 38°. As before, the angular distance, AB, measured with the sextant, is 30°, BC is 30°, will

AC be 60°? D is a fourth star 5° above B; will the angular distance, CD, measured with the sextant, be greater or less than CB; and will the angular distances, CD+DA, measured with the sextant, be greater or less than the angular distance, CA, measured with the sextant? N.B.—A most important problem connected with lunar observations is connected with the above questions.

Here are a few questions to be answered; they are neither catches, nor problems of any unusual difficulty. If a person assuming to be an astronomer cannot answer each, and all of these, without any great mental exertion, he has no claim to be considered anything else than a sort of crammed automaton, who knows nothing of the real science of astronomy.

Certain gentlemen who claim to be profound, and to know a great deal about astronomy, have done a vast amount of strutting when they have referred to my two works. They have shown that they are very strong in their opinions, but absurdly feeble in their arguments. I have had too much experience as an Instructor, to be imposed upon by pretensions, and I now bring the matter to a practical test. Can any of these persons solve the questions I have given, without making use of the information given in my previously-named works? They may attempt to avoid the ordeal, and no doubt they will; but on the correct answering of these questions depends the proof of their knowledge, or if they fail, a display of their ignorance.

There are many other questions depending on the

true movements of the earth, which I have not yet even mentioned; but enough has been given to test whether even an elementary knowledge of the earth's movements has been gained. To assert, as so many men have done, that astronomy cannot give any explanation of the causes, why climatic changes have occurred on earth, and yet to assert that the science of astronomy is perfect, is like hearing a voice from the school of Herodotus, attempting to teach people the exact size and boundary of various continents, when the owner of this voice did not know that the earth was spherical, or that such a continent as America even existed.

CHAPTER XIX.

Relative Value of Problems — Incompetent Judges — Useless Investigations.

SOME thirty years ago, when the stubborn facts brought forward by geologists proved that most extraordinary changes of climate must have prevailed on earth, and that comparatively lately, an arctic climate had extended to about 54° of latitude, attempts were made to obtain from astronomers some explanation of these facts. Astronomical theorists, however, blocked the way, and asserted that astronomy could give no explanation; they declared that no greater change could occur than 1° 21′ in the extension of the arctic circle, *because*—so they declared—the plane of the ecliptic could not vary more than that amount. What, may we ask, has a variation in the plane of the ecliptic to do with the question? The movement is in the earth itself, not in the ecliptic, *i.e.* in the course which the earth traces round the sun. It would be as ridiculous to assert, that the earth could not rotate on its axis because the ecliptic varied its position slightly, as to assert that the earth's axis could not change its direction

except in one way, because the plane of the ecliptic did not vary much. But what are we to do with our facts? We have evidence that during 2,000 years the earth's axis has traced a circle round a point 6° from the pole of the ecliptic, and this same curve carried back to 13,000 B.C. brings the arctic circle down to 54° latitude, as proved by geological evidence was the case. The crammed minds of theorists are not affected by such evidence. It is no proof at all, they assert, it is a mere coincidence, that the curve is such as the evidence defines it to be. We must admit, they say, that the curve is such as you state, as far as the evidence proves it, but beyond that date it moved very differently.

Can we find in the objections of ancient theorists anything more unreasonable than this argument? Probably not, but we can find one exactly similar. The telescope, said the objectors to Jupiter's satellites, is a terrestrial object fitted to look only at terrestrial bodies. It is true, it causes a distant hill to appear nearer, and larger, than when seen with the unaided eye, but immediately you direct it to a celestial object, it deceives you, and no matter what we saw through it, we should be convinced did not exist.

The inquiry as to any possible means of explaining geological changes by astronomy, was obstructed by dogmatic theorists, and some other subjects were started in order to bring astronomy into notice. Fortunately the transits of Venus would occur after a few years, and the rarity of this phenomenon, caused the general public

to form an entirely erroneous idea of its practical importance. The transit of Venus affords the best means of finding the distance of the earth from the sun, and consequently enables us to ascertain what may be termed, the scale of the solar system. To know this scale accurately is a subject of interest, but it cannot be designated one of any vital importance. From the transit of 1769, the distance of the sun from the earth was ascertained certainly to within five per cent., if not to within the limits of a smaller error. Now if the sun were at a mean distance of 90,000,000 miles from the earth, or of 100,000,000 miles, every movement of the celestial bodies could be foretold with equal accuracy under either condition. The knowledge of the actual distance of the sun from the earth, enables us to calculate the exact diameter of the sun, the exact distance of the various planets and their diameters, and it is a subject of interest to know these items. So also it would be of great interest to know to one inch, the distance between the Nelson Column in Trafalgar Square, and the Column in the Place Vendôme in Paris. The practical importance, however, or the benefit to science of such exact knowledge, is not equal to that of knowing what are the movements of our own earth, because the movements of our earth are similar to the movements of an instrument, with which we are making observations. If we are not thoroughly acquainted with these movements, the observations made with the instrument will give incorrect results, and will mislead us.

In consequence of the true movement of the earth, which causes the conical movement of the axis, not having been understood, hundreds of observers have, during the past fifty years, wasted their time by making observations and long calculations, relative to what they imagined was the proper motion of the fixed stars; the inconsistencies which were attributed to a movement in the stars themselves, being in the majority of cases due to the movement of the earth, being different from that which theorists have imagined. One very important fact, however, has been established, viz., the money value that is placed on even so comparatively an unimportant item, as discovering (if possible) the actual distance of the sun. It might be interesting to know how much money was voted for the various parties, who were sent on the two expeditions to make observations on the transit of Venus. Not only did our own Government sanction a considerable sum for these expeditions, but other Governments also paid for similar trips. If, then, we can ascertain what results were obtained from these observations, and what additional accuracy we are certain of, we may at once state that it is worth so many thousand pounds to know that the sun is so many miles nearer to, or farther from, the earth, than was agreed to from the observations made in 1769.

It seems singular, however, that whilst so much prominence and importance are given to this particular problem, and so much money has been expended in the endeavour to solve it, that the movement of the

earth, which I have demonstrated, explains fully the precession of the equinoxes, the decrease in the obliquity of the ecliptic, the reason why this decrease has a decreasing rate, the reason why 15,000 years ago Scotland and part of England had an arctic climate, as proved by geology, and explains why certain stars appear to have a movement termed "proper motion," because this movement does not accord with the theories at present accepted, should be ignored, and kept out of the notices of the Royal Astronomical Society. If it be worth many thousand pounds to know whether the sun is 92,000,000 or 94,000,000 miles from the earth, one would imagine that it was worth something to know how the earth itself moved.

When we perceive that the gold medal of the Royal Astronomical Society was given for a paper containing a list of stars having a proper motion, and this proper motion was supposed to be obtained from a formula, so utterly erroneous that even a tyro in geometry would perceive this error, and when also we find that a leading astronomical authority asserts, that no matter whether the axis of the earth changes its direction in consequence of a swaying movement or of a second rotation, yet the earth would be similarly affected, it induces a belief, that the persons who decided on these two questions, were not quite as qualified to be competent judges, as they imagined themselves to be. When also one is told that a movement of the earth's axis in a circle which, explains so many facts is impossible, because it is a circle,

whilst the accepted theory which does not explain the facts is also a circle, it is difficult to comprehend why a circle is impossible because it is a circle, and why another circle is accurate because it is a circle. If a circle is impossible in one case, surely it would be in another. Is not this something like orthodoxy is my doxy, heterodoxy is another man's doxy with which I don't agree.

There is no doubt that the great astronomical problem of the present day is to discover the cause of the climatic changes which have taken place on earth, and which the facts of geology have proved did occur in the past. The glacial epoch which prevailed comparatively lately, is the first of these great changes to which we can refer. The very same movement of the earth's axis which is now going on, and which it is known has been going on during the past 2,000 years, if carried back to 13,000 years B.C., gives us exactly those conditions of climate which geologists find did occur; this movement also explains many other mysteries in astronomy, and requires no novel theory, but merely the acceptance of facts now occurring, to give the required solution. We have in this problem proceedings similar to those adopted by the detectives in Edgar Poe's amusing tale, "The Purloined Letter." The solution is too simple. Men now are probing the covers of books, examining the floor with microscopes, watching for sawdust on the ground, and dividing the carpet in to squares; in other words, they are repeating observations by the

thousand, on stars and planets, to discover that which, though staring them in the face, they cannot perceive, or cannot accept, because it is opposed to some dogmatic theory, based on the absurd oversight, that the centre of a circle can vary its distance from the circumference, and yet remain always the centre of the same circle.

We have paper after paper printed in scientific journals, giving the results of the observations that have been made, and long descriptions of the adjustment of the instruments used. But what results are obtained from all these records? The proceeding is similar to that of the person at the Woolwich Observatory whom I found keeping fowls, and sometimes occupied with a large staff in filling volumes of books with figures, and eventually bringing forth—nothing of any practical value.

A very long account by the French detectives of the detail examination they carried on with their probes, their gimlets, and microscopes, might be very interesting to other detectives who lost sight of the end, and valued only the means adopted; but how about finding the purloined letter?

It might be interesting to inquire, and if possible discover, what amount of money has been granted during the past fifty years, for expeditions to observe eclipses of the sun, and the two transits of Venus, and then to examine the actual discoveries that have been made in connection with these two phenomena. We could thus ascertain the pecuniary value of these discoveries. It would not, of course, come under the head of scientific

information of any practical value, to have merely an account, however detailed, of how the various instruments were set up, how the line of collimation was adjusted, what was the amount of level error, what were the observations the observers made to determine their longitude and latitude, what they found the rate of their chronometer, what was the temperature, and the height of the barometer each hour. The whole of these facts, although backed up by admirable photographs, published in the illustrated papers, of the gentlemen forming these expeditions, imposingly grouped beside their large telescopes, do not give us anything in the way of a discovery, a fact, or even an idea, that is in the least novel. We might, no doubt, have very beautiful photographs of the French detectives who probed the books, who searched for sawdust, who took off the frames of pictures, etc., and also a detailed description of the microscope they used, and the dimensions of the long needles they thrust into the chairs; but if we were not blinded by all this smoke, we should ask, as practical people, How about the purloined letter? How about the proper motion of the stars? How about the cause of the glacial epoch? etc. If the purloined letter was not found, nor even a guess made as to its whereabouts, then all these wonderful microscopic investigations were useless, simply because they were made in the wrong direction. The learned police superintendent roared with laughter, when it was suggested to him that he had failed to solve the mystery, because the solution was too simple for him.

Edgar Poe was truly a philosopher, who seemed to have gained a thorough insight into the working of the over-crammed, and unreasoning mind.

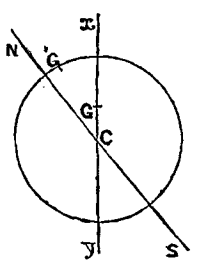
It is most essential that the instrument we use, should be in perfect adjustment, and that we should know exactly what movements it makes, during the intervals between our observations, or whilst we are making our observations. Fix a transit instrument, so that it moves in the plane of the meridian, and we then have the earth itself and the transit instrument, most closely representing a theodolite. In whatever way the earth moves, so does the transit instrument. Each time the earth rotates on its axis, the transit instrument also rotates. If now the earth make any other movement, besides its rotation on its axis, and its revolution round the sun, the transit instrument will partake of this other movement.

It is well known that another movement does occur, this other movement being described in orthodox astronomy, as "a conical movement of the earth's axis."

To me as an observer with the transit instrument, this movement as described, seems very incomplete. The earth consists of something else besides an axis. I want to know how the meridian on which my transit is fixed, has moved in accordance with, and during the movement of, the axis in its conical course. I want to know whether it is the earth's axis, or the semi-axis, that traces the cone. As far as the course of the axis on the sphere of the heavens is affected, similar results would be manifested in either case, but very different results

would occur to my meridian, and to my giant theodolite, according as one, or the other, movement occurred. A very simple explanation will suffice to explain these differences.

Take a small globe, or an orange; through either of these spheres, drive a piece of wire, or stick, to represent the axis of daily rotation. This wire or stick must pass through the centre of the sphere. Drive a second wire or stick through the sphere so that this second wire forms an angle of about 29° with the first wire or stick. The following diagram represents the sphere under these conditions.



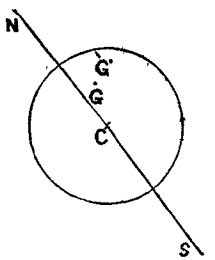
NS represents the axis of daily rotation, *xy* represents a second axis.

If the axis *xy* be held firmly and one rotation be given to the sphere, it will be found that the semi-axis NC will have described a cone, and the line CN produced would trace a circle. By this movement every part of the sphere would trace a circle except the two points *x* and *y*, which would remain fixed as regards space.

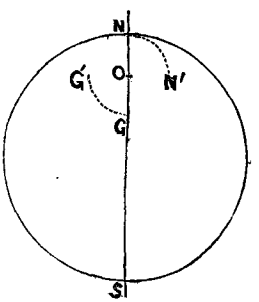
Thus a point G on the earth's surface, would be carried to G′ when one-fourth of this conical movement had been made by the semi-axis, and the meridian on which G was situated, would be displaced from its original position in a most marked manner, as will be more fully explained at a future page.

If we again take a globe, or sphere, with an axis NS,

as in the following diagram, and cause this axis or semi-axis to trace a conical movement by means of a swaying movement, very different results would follow, as may be practically tested with the sphere to which I have referred.

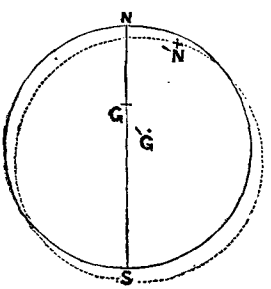


Thus, NS the axis of daily rotation, G a point on the surface of this sphere. Give to this sphere a swaying movement, so that the semi-axis CN traces one-fourth of a cone; the point G will not be carried as before round one-fourth of a circle, but will be transferred to a relative position, such as is shown by G'. The effects of these two movements on various meridians are therefore very different, although in each case the semi-axis has moved in exactly the same manner.



This problem is of such vital importance to the science of practical astronomy, and appears to have been so entirely overlooked and neglected by theorists, that I will venture to give additional demonstrations on the changes of a meridian due to this movement, for the information of those capable of reasoning. Suppose NS the axis of daily rotation, G a locality on the earth's surface. Take O

as a fixed point on the surface of the sphere, and cause the sphere to make one-fourth of a rotation round the axis of which O is one extremity. The pole N will be carried to N′, the point G to G′ by this movement, and the meridian NG would be carried to N′G′. If now a swaying movement occurred, and produced the conical movement of the semi-axis of daily rotation, the effect on this same meridian would be as shown below. The pole N, by the swaying movement, would be carried to N′, G would be carried to G′, and N′G′ would now be the new position of the meridian of which N′G′ is a part.



When we practically test these facts—and it requires no cramming to enable a person to test them—we perceive that, although the axis in each case moves in a similar manner, and the course traced by this axis on the sphere of the heavens, would be alike in both cases, yet the meridian of localities on earth would be differently affected according as one or the other movement occurred.

When we know that the right ascension of various celestial bodies is determined by their meridian transits, that any apparent irregularity in these transits is attributed to some proper motion in the stars themselves, and consequently that it is essential that every possible change in the direction of the meridian should

be understood, it is astounding that leading astronomical authorities should assert, that, no matter whether the conical movement of the earth's axis is produced by a slow second rotation, or by a swaying movement, yet the effects on all parts of the earth will be the same.

I will now appeal to those readers who are capable of reasoning, and will ask them to attempt the following practical experiment. Take a small globe, and place this globe in a fixed box which fits very closely to the sides of the globe. Then endeavour to give a conical movement to the semi-axis of this globe in any other way than by giving to the globe a second rotation. The globe should be so placed in the box, that the line joining the north and south poles, forms an angle with a vertical line. If the experimenter can cause the semi-axis of this globe to trace out a conical movement, whilst the globe remains in the box, he will know that the authorities are correct who assert, that the movements are exactly the same, no matter whether a swaying movement, or a second rotation, produce the change in direction of the axis. If he can only produce a conical movement of the semi-axis, by giving the sphere a second rotation, he will come to a different conclusion.

Trifling and simple as this experiment may appear, yet depending on it is a matter of some interest, and if we,are disposed to expend many thousands of pounds, in endeavours to discover whether the sun is 92,000,000, or 94,000,000 miles from the earth, we may fairly con-

clude that it is some value to know, that the earth has a movement which has never before been known.

I now state that, just as it is impossible to give to a globe in a fixed box, a conical movement of its axis, by a swaying movement, without removing the globe from its box, so it is impossible that the axis of the earth ever performs a conical movement by a swaying movement, without moving the earth out of its orbit. That the conical movement of the axis does occur is a fact, proved by the recorded observation of 2,000 years, and the only possible way of accounting for this is, that the earth has a slow second rotation round a second axis. The facts are awkward for theorists, but nevertheless these facts exist.

The velocity with which the earth revolves round the sun causes the *centre of gravity of the earth* to move round in a uniform course; it is the same as though this centre of gravity was upon a single rail. If the change in direction of the earth's axis were due to a swaying movement of the earth, then this swaying movement would move the earth out of its orbit by several miles, and would be similar to suddenly removing a body from off its rails. If, however, the change in direction of the axis be, as it is, due to a second rotation of the earth, then the earth maintains a uniform course in its orbit, the centre of gravity, and the earth itself follow year after year the same course with minute accuracy, and yet the conical movement of the axis occurs during the revolution of the earth round the sun, and its diurnal

rotation on its axis. If, as has been asserted, the conical movement of the axis be due to a swaying movement of the earth, then any person who cares to test the accuracy of this statement, can cause the axis of the sphere which is placed in a box, or in a narrow tube, to make a conical movement; if he cannot perform this operation, he need not be crammed with much stock knowledge, to realise the fact, that the assertion of the swaying movement occurring, is merely the result of the persons who so assert being unacquainted with the most simple facts in dynamics.

During one complete revolution of the earth's axis round that point in the heavens which is the centre of polar motion, and which is now 29° 25′ 27″ from the pole, a movement equivalent to one rotation of the earth occurs round this second axis. Consequently, during the thousands of years which pass during this second rotation, there will be one day less than would occur, if a mere swaying movement of the earth, occurred and produced the conical movement of the axis; it follows, therefore, that it is impossible to define with minute accuracy, in what a rotation of the earth consists, unless it is known exactly, how this second rotation affects each locality on earth. When, however, we find that it is stated that the results would be the same, no matter whether the conical movement was produced by a swaying movement, or a second rotation, what are we to think of the knowledge on this subject of those persons who state that the moon's mean motion is increasing, when they cannot

know in what a rotation of the earth really consists? When, also, a different portion of the earth is brought each year into conjunction with the sun and moon, according as the one or other movement of the earth occurs, how can it be asserted that eclipses occur more and more to the eastward than they could occur, unless the moon were moving more quickly? We must always have a standard measure of time, in order to make such assertions, as that something else is moving more quickly; and how can we possibly have such a standard, unless we know what is the actual movement of the earth, which combines with the conical movement of the axis? The conical movement of the earth's axis is a result, not a cause; it is the result *of the earth having a second and slow rotation, round an axis inclined at an angle of* 29° 25′ 47″ *to the axis of diurnal rotation.*

It is the old story of three hundred years ago repeated. A movement of the earth occurs which is neither known nor suspected; results due to this second rotation occur, which are attributed to half-a-dozen different causes; nearly every star is said to have a proper motion. The plane of the ecliptic is said to move; the moon is said to be moving quicker. The centre of a circle, it is asserted, can vary its distance from its centre, and yet remain the centre of the same circle. The record of the arctic circle having reached a parallel of 54° latitude, written as it is on the rocks, and testified to by every boulder, carried from Westmoreland, and Cumberland, into more southern

regions, is mysterious and unaccountable by the astronomy, which is unacquainted with the second rotation of the earth. Herodotus, also, was unable to account for the fact announced by the Phœnicians who rounded the Cape, viz., that when on the coast of South Africa, the sun rose on their right hand, and moved from right to left, and set to their left. It was mysterious to him, and could not be accounted for on the theory of the earth being a flat surface, any more than the arctic climate reaching to 54° latitude, no longer ago than 13,000 B.C., can be accounted for by those persons, who do not understand the true movements of the earth.

Not only are the above-mentioned facts explained by the true movement of the earth, but others, at which I have not in these pages even hinted, receive an immediate solution from the same cause, though a separate theory has had to be invented to account for each effect, just as a fresh cycle had to be invented, to account for facts, when the theory of Ptolemy was considered orthodox astronomy, and when the earth was supposed immovable.

I will now ask any person possessed of reasoning powers, to examine the following three movements of the earth:

First, a revolution of the earth round the sun during one year, the plane along which this revolution occurs being inclined to the earth's equator at an angle of about 23° 27′ at the present time.

Second, a rotation of the earth round its axis during each day.

Third, a second slow rotation during many thousand years round an axis inclined to the axis of daily rotation at an angle of 29° 25′ 47″, the north pole of this second axis being located at about eighteen hours and a few minutes of right ascension.

Every fact connected with the precession, with the change in polar distance, and right ascension, of the stars, will be explained thereby. Also why the obliquity of the ecliptic decreases, why this decrease has not taken place uniformly, why the arctic circle reached to 54° latitude during the glacial epoch, why the actual right ascension of stars, differs from their calculated right ascension, and why, consequently, they have been supposed to have a proper motion, which does not occur.

All these mysteries at once disappear, and will be seen to disappear, if any person will merely examine the fact for himself, and apply the advice so applicable to the case, and given in the "Transactions of the Medical Society," page 246, in the following words:

"One of the most formidable obstacles to the advancement of science has ever been a blind submission to authority. To untrammel the mind from the influence of mere authority, that it may have free scope in the investigation of facts and laws which exist, and are established in nature, is the grand antecedent necessary to scientific discovery and permanent progress."

CHAPTER XX.

Statics and Dynamics—The Moon's Rotation on an Axis—The Earth's Axis and the Second Rotation—Facts Explained.

FROM the experiences which I gained during the fifteen years that I was employed in instructing, I found that whilst a large percentage of individuals could readily comprehend a problem connected with statics, or bodies at rest, a small percentage only seemed capable of understanding a somewhat similar problem, connected with dynamics, or bodies in motion. Place on a billiard table say ten billiard balls, and let these be at rest. Any average intelligence will count these balls rapidly, and give the correct number. If, instead of being at rest, we cause these balls to move about, a difficulty is at once experienced in counting them. It appears as though the movement of these balls prevented their grouping, and consequent numbers from being photographed, as it were, on our perception. There is something more than mere wit in the remark attributed to the Irishman, viz., that he could count all his pigs but one, and this one was a little speckled fellow that kept running about so, that he could not count him.

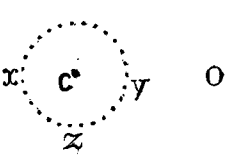
In chess each piece has a certain value, and this value is a constant. Chess may be termed a statical game. In whist, however, each card has not always the same value. The knave, which at the commencement of the play of a hand, is the fourth best card of a suit, may after the first round of this suit become the best card. As the play of the hand continues, the cards are continuing to alter their value, and thus whist may be termed a dynamical game. I have had considerable experience of both these games, and among those who have practised them, I have found twenty, who could play a fairly good game of chess, but I have not found five who could play a fairly good game of whist. Statical chess is more easily comprehended than dynamical whist.

When we come to problems of a larger description, the same intellectual defects are manifested. The evidence appears to be sound, that upwards of two thousand years ago, Pythagoras taught that the earth rotated on its axis, and revolved round the sun, but the feeble intelligence of the orthodox astronomers, at that date, could not grasp this dynamic problem. The usual system of burking the facts, and abusing the discoverer, was therefore adopted, and the earth remained—so it was stated—stationary, until Copernicus revived the theory of the earth's rotation, and after Copernicus, came Galileo, bringing forward the same problem. The strongholds of error are not easily reduced, especially when men have been so crammed, that their reason seems

incapable of acting, so that the system of astronomy "with epicycle and eccentric scribbled o'er," in which no less than seventy-nine spheres were required to explain the movements of the celestial bodies, was considered a grand and profound science; whilst the rotation of the earth on its axis, which did away with these spheres, was asserted to be the most absurd delusion that had ever been brought forward.

We have in this another example of the inability of a statical mind to comprehend a dynamical problem.

During many years I have met individuals by the score, who would affirm that they could not understand that the moon rotated on its axis. To these persons the assertion seemed impossible. As an explanation to these individuals, I have adopted the following proceedings.



I have caused the sceptic to stand some yards from me, say at O, I have stood at *x*, and have turned slowly round.

"Have I rotated on an axis?" has been my question.

"Certainly you have," has been the reply.

I would then walk slowly round a small circle *xyz*, whilst at the same time I rotated as before.

"Have I now rotated on an axis?"

"Of course you have."

"Be quite certain that I have rotated," has been my caution, whilst I repeated this movement several times.

"Of course you have; but the moon——" And the

individual wants to rush off, and entangle his brain with some erroneous ideas of his own. Stop him, and keep to the question, Have I rotated on my axis?

He will admit that you have.

"If now you walked out of the room, and I continued to move as before, the fact of your having altered your position, would not prevent my movement still being a rotation on an axis?"

"Of course not."

"If you came close to me, or stood anywhere in the room, the fact of your having moved, would not alter my movement being a rotation on an axis?"

"Certainly not."

"Then come and stand at C, in the centre of the small circle that I walk round, during the time I rotate on an axis, and what happens? Is not my face always turned towards you during this rotation?"

"Yes."

"Could I keep my face always turned towards you if I did not rotate?"

"No."

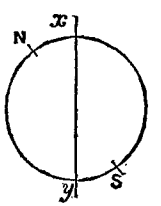
"My movement now is exactly similar to that of the moon, which keeps one face always turned towards the earth, and in order to keep one face always turned towards the earth, the moon must move as I do, viz., rotate on an axis, once during the time I move round you."

Out of about one hundred persons on whom I have practised this demonstration, about sixty have said that

they now saw the case clearly, and that the moon must rotate on its axis in order to keep the same "face" towards the earth. The remaining forty would hesitate for some time, would look puzzled, and would then state that they could not perceive even now, that the moon rotated on its axis, and they could not believe it did.

I have found this to be one among several problems which are admirable tests of the capacity for reasoning possessed by various minds. Some men cannot comprehend it, any more than a baby could understand a problem in Euclid. And if these men held official positions, where they had to accept, or reject, any novelty, they would to a certainty decide that the rotation of the moon on its axis was an absurdity, and a delusion, and they would imagine that in giving this decision they were acting conscientiously.

I will now pass on to another case connected with rotations. Take a small globe, and hold this globe so that the axis is inclined to the vertical, as shown below, NS being this axis. Now turn slowly round, not altering the angle which the globe's axis makes with a vertical line *xy*. The fact of your having turned round whilst you hold the globe in your hand, has caused this globe to rotate once round the axis *xy*.

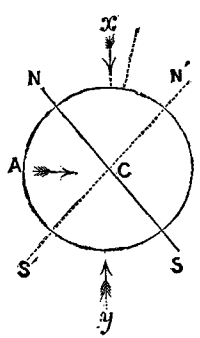


Now give the globe a spin round the axis NS, and then turn round as before. You now have two rotations

in this globe, one a rapid rotation caused by the spinning round NS, the other the same slow rotation round a vertical axis *xy*.

When the globe did not spin, the slow rotation occurred round an axis which might be said to pass through two points on the surface of this globe. When the globe is spun, and the two movements are combined, the same results occur exactly, but that a second rotation occurred would be incomprehensible to the same class of minds that could not perceive that the moon rotated on its axis.

When we wish to examine any particular movement in a body, which has several other movements, we can best do so, by neglecting these other movements for the time being, and deal only with that movement which we are at the time investigating. Adopting this method in the present case, only one movement of the earth will be examined, viz., its slow second rotation, and the effects of this movement alone will be demonstrated. Suppose NS the axis of daily rotation of the earth, *xy* the direction of the axis of slow rotation. Take a date, say the 21st December of any year, and note the direction of the axis NS. After 365 rotations of the earth on its axis of diurnal rotation have occurred, the second slow rotation of the earth will have caused the points N and S on the earth's



surface to have turned a small arc, this small arc being a portion of a circle having for its centre the point where x and y strike the sphere. By observation we know that this change in direction of the axis NS amounts to about 20″ annually. As this slow rotation occurs, the axis is gradually carried round until, when the half of the slow rotation is completed, the axis which occupied the position NS now occupies the position N′S′. The semi-axis CN has traced half a cone; the pole, located by the production of CN to the sphere of the heavens, has traced half the circumference of a circle round a point as a centre, which centre is located by the production of yx to the sphere of the heavens.

The movement which here occurs as regards the semi-axis of daily rotation, and due to the slow rotation round a second axis, is exactly similar to that which occurs to the zenith of a locality on earth, during half a daily rotation round the axis of daily rotation. Just as the zenith traces a circle round the pole of the heavens as a centre, during each twenty-four hours, so will the pole of the heavens trace a circle round its true centre, during one slow rotation round its second axis. That which occurs to any given zenith during each daily rotation, also occurs to *the zenith of the poles* during one slow rotation. The two movements are very similar, though the results are very different. In each example a circle must be traced, if the position of the centres of each is unchanged.

It is amusing to note how entirely unacquainted,

certain persons who claim to be authorities, must be with matters relating to rotations, when they assert that my problem must be incorrect, because I stated that the pole of the heavens traced *a circle* in the heavens, their argument being that no such thing as a circle could be traced, *because* the planets moved in elliptical orbits. A display of such ignorance is not likely to alter the judgment of those, who have studied the question, and who know, that the zenith of each locality on earth traces a circle each time the earth rotates, and that the pole of the heavens must also trace a circle in the heavens, during each slow rotation round the second axis. To describe the rotation of the earth "as a conical movement of a line joining the earth's centre and a given zenith" would be rather an obscure definition, and I cannot but think, that the present known movement would be better comprehended, if it were described as a second, and slow rotation, round a second axis, than to give the vague statement, that a conical movement of the earth's axis occurs.

The second rotation of the earth which occurs, and which is the cause of the semi-axis tracing a conical movement, presents many beautiful and interesting problems. Those persons who are capable of reasoning, and who care to follow out this movement, will find plenty of detail matters, to which they may devote their attention. The change in direction of the earth's axis, produced by the second rotation, explains all the known

facts, connected with the precession of the Equinoxes, and the changes in polar distance of various stars. But in addition, this second rotation explains why the obliquity of the ecliptic decreases, why it decreases now at the rate of about 45″ per century, why it formerly decreased at the rate of 66″ per century, why the arctic circle formerly extended to 54° of latitude, why it appears from the supposed "proper motion" of stars, that the whole solar system is rushing towards the constellation of Hercules, at a rate which is almost miraculous, and why this belief is a delusion. It will also enable a reasoner to perceive, why the erroneous assertion has been made that the moon is moving more and more rapidly, and consequently must sooner or later, tumble down on the earth. It will be also evident, why the present knowledge of theorists does not enable them to know in what a rotation of the earth really consists, and consequently that a true standard measure of time is not known, and it will explain why a few minutes of time had to be fudged in some years ago (as stated at the end of Herschel's "Outlines of Astronomy") in order to make facts and theories agree.

The reader who calmly examines the results of a second rotation of the earth round an axis inclined 29° 25′ 47″ to the axis of diurnal rotation, will be able to solve not only all the problems now known to astronomers, but also several others, the solution of which is to them inexplicable; in addition also there are other

items, which at present are scarcely imagined to exist, but which, when discovered, will be as inexplicable as the climate of the glacial epoch. The explanation of these is not to be found in the theories at present accepted, but in an examination of this second rotation, taken in its full details.

That there may be no obscurity about my meaning, I now claim that the earth, in addition to its daily rotation, has a slow rotation, completed during 31,850 years, round an axis, the poles of which are located 29° 25′ 47″ from the poles of daily rotation. The North Pole of this second rotation is located 29° 25′ 47″ from the North Pole of the heavens, 6° from the pole of the ecliptic, and having a right ascension of about 268° at the present time. The annual rate of this slow rotation is 40″·03, for the great circle most distant from the poles of the second rotation. The North and South Poles of the daily rotation are carried annually by this second rotation over arcs of 20″·158, thus causing the precession of the equinox. The course of the pole of daily rotation is a circle, round the pole of the axis of slow rotation. This second rotation is the cause of great climatic changes on earth during long periods, and explains many other mysteries.

CHAPTER XXI.

Influence of Authority — Arrogance — Feeble Reasoning — Amusing Arguments—Cramming, a Defect.

During the history of scientific progress there has probably never been any age, during which the influence of scientific authority has been greater than it is at present. Let any novelty be brought forward, and if this novelty cannot be proved by experiment, it is at once referred to "authority." This authority may be as incompetent to judge of the accuracy of the problem, as was the senior officer who stated that my demonstration of the parallax of the sextant was "all wrong," and who shortly afterwards admitted that he had made a mistake. When, however, the authority, with a Pope-like claim to infallibility, has pronounced his opinion, the mass of half-trained men of science merely follow him like a flock of sheep. To venture to offer an opinion in opposition to such an authority, is not to be thought of. When I read before the Geological Society some fifteen years ago, my paper explaining the cause of the last glacial epoch of geology, there were gentlemen present whose observations and writings thereon were

known wherever the English language is read. When, however, I put the question as to whether the accepted theory was correct, that the centre of a circle could vary its distance from its circumference, and yet remain the centre of the same circle, there was no man bold enough to state, that no matter whether I was right or wrong, yet the then accepted theory must be wrong.

The replies were: It is an astronomical question, we cannot offer an opinion, mathematicians must decide. It certainly explains our facts, but we must defer judging it, till we know whether it interferes with the accepted theories of science.

Here was the blind adherence and submission to authority, which has ever been one of the most formidable obstacles to scientific progress in all ages.

It was this same cause which prevented the rotation of the earth from being admitted as a fact, at the remote date when Pythagoras taught it. It was the same cause which led to a neglect and rejection of the same fact, when Copernicus brought it again into notice. It was the same blind adherence which caused Galileo to be persecuted, and prevented people from even believing their eyes, when they looked through a telescope and saw that Jupiter possessed satellites. How could they tell that their eyes did not deceive them, when scientific authority announced that Jupiter could not possess satellites?

There is a vast difference between obedience to well-established laws, and a blind submission to mere asser-

tions, which, without any sound evidence to prove their accuracy, are believed, merely because an authority makes them. So also there is a vast difference between modesty which is displayed by an individual, who, never having examined, or reasoned on a subject, does not feel qualified to express an opinion, and timidity which is shown when a person observes a fact, with his own eyes, but is afraid to express his conviction that he did see this fact, because authorities have asserted that the "fact" could not exist (Jupiter's satellites, for example). Then, again, there is a great difference between boldness, and arrogance. Any individual possessed of the slightest knowledge of geometry, must perceive that the two statements, viz., that the pole of the heavens traces a circle round the pole of the ecliptic as a centre, and never varies its distance from this centre, and yet the obliquity of the ecliptic has slowly decreased, are contradictions of each other. It might require some boldness to state that, in spite of all the astronomical authorities having accepted this impossibility as correct and satisfactory, yet it could not occur.

When, however, we find a writer asserting that the pole of the heavens cannot trace a circle, and because I have defined the course of the pole as a circle, I am entirely wrong, and show that I know nothing of astronomy, it is not boldness, but arrogance, that offers this opposition; and it is also an example of the arrogance of ignorance, because had this person known

anything of the subject on which he had set himself up as a judge, he would have known that the accepted theory was, that the pole of the heavens *did* trace a circle, though not quite the same circle which I had demonstrated was really traced.

When we examine the details of events which have occurred in the past, we can usually form a more impartial opinion on these, than we can when the events are taking place around us. It is something like looking at a battle from a distance, and being in the midst of the fighting. The distant observer sees more accurately what occurs, than the man in the thick of the fight.

When we examine the objections which have been urged in past times, against every novelty which was in opposition to the authority of the time, we at once perceive the absurdity of the objections that were raised. We may omit all reference as to whether the novelty was a truth or a falsity, as having nothing to do with the arguments supposed to *prove* it false. That which I wish to bring into notice, is the total absence of reason displayed by the objectors.

When Galileo discovered the satellites of Jupiter, objectors asserted that the telescope held good for terrestrial objects only, but when turned towards the heavens it deceived the eyes.

When Huygens discovered one of the satellites of Saturn, he asserted that there could not be any more satellites belonging to that planet, *because* their number

was then exactly equal to the number of the primary planets, viz., one for the earth, four for Jupiter, one for Saturn, making six. Mercury, Venus, the Earth, Mars, Jupiter, Saturn, making six. Q.E.D.

Such an assertion was at once bowed to by the submissive. Huygens was an authority, and consequently what he stated was not to be questioned. At this distant date men presume to laugh at the absurdity of such arguments, and wonder how these old gentlemen could have been so stupid, as to imagine that their objections were sound; but have any of these individuals endeavoured to realise what their views, or remarks, would have been, had they lived at the dates to which I refer? If, however, they are so very wise *after* the events, how is it that when a venerable astronomer stated that my paper (read before the Royal Astronomical Society, in which paper I pointed out that the centre of a circle was now supposed to vary its distance from the circumference) was of no value, *because*, so he stated, he found in Herschel's "Outlines of Astronomy" that the pole of the heavens never varied its distance from the pole of the ecliptic, but yet the obliquity varied, and these two poles must therefore vary, this objection was considered unanswerable?

Why should we laugh at the absurdity of a false objection, when urged two or three hundred years ago, and accept as profound an exactly similar objection put before us in the present day?

The reader who has comprehended the results which

would occur in consequence of the second slow rotation of the earth, will perceive that this second slow rotation must cause the semi-axis of daily rotation to trace a cone during one slow rotation. If the semi-axis trace a cone, it is impossible that the axis produced to the heavens can trace any other curve than a circle.

Yet what do we find as an objection against this fact? It is asserted that the movement cannot occur, because a circle is spoken of, and no circle can be described in the heavens either by a zenith during a daily rotation, or by the pole during a slow rotation, because—why?—*because the planets move in elliptical orbits.* A wise and profound argument this; and affording an excellent example of the reasoning powers of the objector!

Even Mr. Belt, whom I found clear-headed on most questions, became confused when this objection was urged. He asked me whether I could not find evidence of the curve being an ellipse very near in form to a circle. I endeavoured to explain to him, that there was no connection whatever, between the conical movement of the axis of a rotating planet, and the movement of this planet in space. I pointed out that the conical movement of an axis or semi-axis must, as a geometrical law, cause the pole of this axis to trace a circle in the heavens, just as each zenith traced a circle each time the earth rotated; but it was not until after some months of these arguments, that he admitted the objection to be absolutely absurd. He then perceived that to assert that the

semi-axis of the earth traced a cone, whilst the pole due to this movement could possibly trace an ellipse, belonged to the same class of intelligence, as asserting that the pole of the heavens traces a circle round the pole of the ecliptic as a centre, and never varies its distance from this centre, and yet the obliquity of the ecliptic is gradually decreasing.

There is another form of argument used by persons incapable of reasoning, and who have a superficial knowledge of science, when they wish either to raise an objection against a novelty, or to explain a fact, the cause of which they do not understand. The first example I experienced of this form of argument was when among the Caffres.

In order to surprise some Caffres who had visited me, I produced a magnet, and placing a needle carefully on some water, so that it floated, I concealed the magnet in my hand, and attracted the needle towards me. The Caffres were more than surprised, and accused me of practising witchcraft. I then explained to them that it was not witchcraft, but a power termed magnetism, which attracted the needle.

Some days afterwards, I happened to open a bottle of soda-water in the presence of several Caffres, among whom was one who had been present when I explained the magnetic power. As soon as the Caffres saw the soda-water bubble, they thought it was boiling, and when I drank it they exclaimed that it was witchcraft. "Not witchcraft," said the Caffre, who was eager to display his

lately-acquired knowledge, "it is magnetism which enables him to make water boil without a fire, and then to drink it whilst it boils."

We may laugh at this Caffre, and may consider him an example of gross ignorance, but what are we to say when we find a similar class of argument employed by a leading scientific authority? When I pointed out the fact relative to the "pointers" not always pointing with equal directness towards the pole star, and explained this fact by the geometrical laws affecting it, an authority asserted that he should account for it by "refraction." Had he said by magnetism, his argument would have been equally as applicable. Refraction causes bodies to appear slightly higher in the heavens than they really are, but when a certain star is at a given altitude, no matter whether to east, or west, of the northern portion of the meridian, the refraction for this star will be the same, in the same state of the atmosphere; but when the pointers are on one side of this meridian and at a given altitude, or on the other side and at the same altitude, they in one case point directly at the pole star, in the other case they do not. Refraction has as much to do with the explanation, as magnetism had to do with soda-water bubbling.

The reasoning powers of the Caffre I have referred to, were no worse, and no better, than those of the scientific authority. Neither of these men was capable of perceiving the connection, between cause and effect. Yet one was what would be termed an uneducated

savage, the other was a man crammed with scientific facts.

What do we learn from this example? It proves that no amount of stock knowledge acquired by cramming, enables the mind to reason, when a novelty is placed before it; and if such be the case, as I affirm it is, then the extensive cramming which is now the fashion, not only fails to produce reasoners, and practical men, but tends largely to make men mere automatons, incapable of reasoning, or moving out of a groove.

CHAPTER XXII.

Astronomical Problem—Geological Facts—History Repeats Itself—American Notice.

I HAVE given many pages to a description of a problem to which I devoted upwards of ten years during my employment at Woolwich. I have done so for two reasons. The first of which is as follows :

This problem is one of great importance to the science of astronomy. It fully and clearly explains known facts, which facts cannot be explained by the present theories of astronomy. It requires no new cycles, or epicycles, to be scribbled over the numerous theories now in fashion, but deals simply with a movement known to exist, and with geometrical accuracy. Whereas the present accepted theory starts with a geometrical impossibility.

The movement which I have proved occurs, gives the date of the last glacial period on the earth, viz., about 13,000 B.C. Thus the past history of the earth can be read by aid of this problem. We know, when this problem is understood, that the last glacial epoch prevailed during about 12,000 years. That the enormous boulders

that we find were carried from one part of England to another, were thus carried by icebergs formed in England about 13,000 B.C. We know why the glaciers in Scotland and Wales, and the North of England came gradually down from the mountains, reached a maximum, and then gradually retired; the last glaciers having been in existence about 7,000 B.C. We perceive the reason why the tropical regions yield no evidence of ever having suffered from severe cold, and why it is in middle latitudes that the greatest effects are visible. It is manifest why evidences of the antiquity of man are not to be found far north and south of the tropics, because the alternations of climate during these 12,000 years were so great, that the conditions were not so favourable in middle latitudes, for his habitation, as they were near the tropics. We perceive that the facts brought forward by geologists are facts, and not delusions or vague theories, and we can stamp a greater value on the science of geology, in consequence of its having stubbornly adhered to its evidence, in spite of the ridicule of arrogant ignorance. But above all, we know that astronomy can solve these problems, and give to them a full and clear explanation, when the true movement of the earth is comprehended.

Not only are these facts in the past history of the earth explained, but the mysteries of astronomy itself are solved by the same movement. What, let us inquire, are the obstacles in the way which prevent the true movement of the earth from being admitted? Are the

facts of geology to remain without a solution because dogmatic theorists, who cannot explain the mysteries in their own science, raise objections which are worse than puerile. Any advance which is now to be made in astronomy, must be by reasoning, and by noting what the recorded facts prove; not by employing scores of observers and computers at great expense, to observe and record observations by the thousand, and to fill volume after volume with these; each later volume giving different evidence from that which has preceded it. If the true movement of the earth were understood, a prediction could be given for 100 years in advance, or, for the past, with minute accuracy. Can any astronomer answer the questions which I have given on page 243 of this book? If he can, he must make use of the problem I have demonstrated. If he cannot, his opinion is not of much value.

Here, then, is one reason why I have given so many pages to this one problem, and the second reason is the following:

I have had practical experience that history repeats itself; and, comparing very small things with large, I will give examples.

During the year 1851 I proposed that, instead of firing one or two trial shots with guns to discover the range, angles should be measured with a sextant, and the range found before firing. I was informed that range finding was of no practical use, and was not so good as a trial shot. Some years afterwards it was

decided that finding the range first by instruments, was essential, and that a trial shot was a mistake.

When the Armstrong gun was introduced, I proposed a method for finding ranges from an elevated battery on the coast. I was informed that such a system was not likely to be of any use. When, however, an Austrian officer, some years afterwards, introduced the same method into the Austrian service, it was considered a valuable discovery, which ought to be at once adopted by us.

About the year 1866 I again brought range finding into notice, and proposed two methods, one to use an instrument with a fixed angle, and to have a base variable according to the range; the other, to have a fixed base, and a variable angle. Neither of these methods was adopted.

Some fourteen years afterwards an officer proposed the same method, viz., an instrument with a fixed angle. The method is said to be most valuable, and is at once adopted by authority.

Another example, in a very small matter.

Early in the year 1879 I published a book entitled, "The Art of Practical Whist." In this book I proposed an innovation which I had practically tried during many years previously, viz., that whilst with a suit of five, the penultimate was led, with a suit of six, the antipenultimate should be led; so that in each case the fourth best card should be the card led.

Reviewers of my book passed over this novelty with

somewhat of a sneer. Time elapsed, and whilst stationed at Halifax, several American whist players wrote to me on this subject, and announced that they intended to adopt it. This was during 1879, 1880, and 1881. About the end of 1884 "Cavendish," the great authority on whist, wrote to *The Field* announcing that a most valuable and important discovery in whist, had been introduced *by the Americans*, and he termed this "American leads," which is, to lead the fourth best card of a suit, so that the penultimate should be led with five, the antipenultimate with six in the suit.

During the present year (1885) these leads have been considered of sufficient importance to induce the same author to write a book on the subject.

It is interesting to find one's own children thus looked on as possessing certain merits, but one certainly may have some grounds for objecting to other persons claiming to be their papas.

In the year 1873 I published a book entitled "The Cause, Date, and Duration of the Last Glacial Epoch." In 1874 I published a sequel entitled the "Cause of the so-called Proper Motion of the Fixed Stars."* In these two volumes I explained the true movement of the earth's axis, and showed how this movement explained the cause of the glacial epoch, etc., etc. The book was criticised mainly by somewhat vulgar and personal abuse of its author, a most distinct indication of the inability

* Published by Chapman & Hall.

of the writers to offer any sound or logical objection against the facts and problems given in the two books. By arguments, some of which I have quoted in this work, and which are admirable examples of ignorance of the subject, and an incapacity for reasoning, and by a sort of pompous assumption of superior knowledge, which is not likely to take in any one who is at all acquainted with human nature.

Eleven years having elapsed, I have lately had forwarded to me an American paper, in which a writer states, that in my two works, there seems to be one of the most important problems connected with astronomy, and geology, ever brought before the scientific world during the past century. This writer has evidently, from what he has written, read the books, and has understood them, whereas I do not think it possible that some of my home critics can have done so, at least I hope for their reputation as reasoners, or even competent critics, that they reviewed the books without having read them.

Judging, however, from my past experience, I anticipate, that before long we may have it announced by some astronomical authority, that a most valuable and original discovery has been made *in America*, in connection with astronomy—viz., that the earth has a second slow rotation round an axis inclined at an angle of 29° 25′ 47″ to the axis of daily rotation. That this second rotation causes the axis of daily rotation to trace a conical movement in the heavens, and thus explains the precession of

the equinoxes, the changes in polar distance of the stars, the reason why the obliquity has decreased during the past 2,000 years, the reason of the climate of the glacial epoch, and why the solar system has been supposed to be rushing in the direction of the constellation Hercules, and explains many other mysteries. If, such a writer adds, "considering how simple the solution of these mysteries are, when we examine it, we are astounded how no one ever thought of it before the Americans," he will be acting quite in the conventional manner.

When one has devoted more than ten years to the investigation and solution of a problem, and ten more years in endeavouring to find a flaw in it, which flaw does not exist, he naturally does not feel disposed to allow a mere copyist, to appropriate as his own, that which belongs to another.

I trust that not only every reasoner, but every honest man, will admit that I have some excuse for having given so many pages to the elucidation of a problem, which has never yet been answered, or proved incorrect in one single item, however much the problem and its author may have been abused by unqualified judges or interested opponents.

CHAPTER XXIII.

Intellectual and Physical Power—Individuality—Forms of Heads—Health and Brain Power—Crammed Minds *versus* Reason—Academy Rules Altered.

SOME time previous to my leaving Woolwich, I received from the late Mr. Charles Darwin a communication, requesting to know whether I had not collected some interesting statistics relative to the combination of intellectual, with physical powers, among cadets. I had collected various data in the following form:

I had a list of those cadets who had distinguished themselves in either mathematics, geometry, surveying, practical artillery, drawing, or languages. I had a description of these cadets, as far as their height and general development was concerned; the colour of their hair, whether stout or thin, whether of active athletic habits, or the reverse; the preparatory schools at which they had been trained, before joining the academy; the shape of their heads as regards the mass before, or behind a vertical line, passing through the centre of the ear; the shape of the hands, and, where possible, an approximation to the number of inches round the head.

From these statistics I endeavoured to draw conclusions as regards the mental powers of those cadets whom I saw for the first time each half year. There was one item, however, which to some extent interfered with my forming an entirely unaided judgment—viz., that the cadets took their seats in the class-room, according to the order in which they had passed. I thus knew who had passed first, who second, and so on. Thus my evidence held good, only so far as this: that I knew who had passed first, and I could then note the peculiarities of this cadet, but my means of predicting who ought to be first, when I saw a number of cadets, and then finding whether my prediction was correct, were limited. I often wished, after I had gained considerable experience on certain details, that I could carefully have inspected a class before I knew how they had passed, and then endeavour to place these in the intellectual order in which I believed they might have been. This would have proved whether the experience I had gained had led me to form just conclusions. As it is, I can give some information which every person may not have gained, for few officers in the army have had under their instruction during four months, in batches of from thirty to forty, upwards of eleven hundred young men, varying from the ages of eighteen to twenty-two, and the successful competitors (at an examination) from the principal schools and colleges in England.

As might be expected, those cadets who had distinguished themselves intellectually, were also those who

seemed to enjoy the best health. Good health is, I believe, one of the most necessary items to the full manifestation of intellect; and I will venture to offer a few remarks on this subject further on, because, although not a physician, I think I can claim some reasons for being a competent adviser on the subject of health.

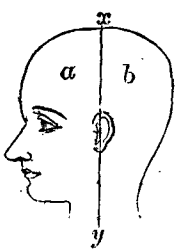
I could not find that great intellectual power was usually combined with either muscular or athletic development. The best runners, or jumpers, among the cadets, were more often among the lower, than the upper half of a class.

Height, or the reverse, appeared also to give no indication of the mental power. The old proverb, that the upper storeys of tall houses are usually weak, when applied to the heads of tall men may be true, but it was not so proved to me, very tall cadets, in more than one instance, being head of the batch.

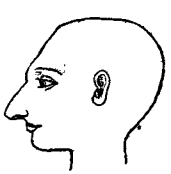
Light-haired, even to redness, or dark-haired cadets, were scattered all over the class; the colour of the hair, therefore, had to be ignored, as an item in the equation by which intellect was to be judged. I did, however remark, that in almost every instance, those cadets who were near the top of the batch, had fine hair, not the coarse, thick hair which approaches the negro type. Now and then an exception was seen, but it was an exception, not the rule.

Without going so far as to admit the accuracy of the whole system of phrenology (true though it may be), yet the general shape of the head was, I found, the only

infallible means of forming a judgment, without other evidence, of the ability or stupidity of a cadet. I could judge more readily by seeing the head in profile, than by observing it full face. In profile I would in imagination draw a vertical line passing through the centre of the ear, as shown by *xy* in diagram. The mass which was in front of *xy* I termed the intellectual, the mass behind *xy* the animal. Thus *a* represents the intellectual, *b* the animal. In almost every instance I found that the cadets at the top of the class, had a preponderance in the *a* department; those at the bottom of the class, either an equality or a preponderance in the *b* portion.



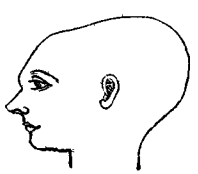
The facial angle also gave most remarkable examples. Whenever the forehead and nose were located in nearly parallel planes, as shown in the sketch below, eccentricity was usually exhibited, and an almost total absence of reasoning powers. If the perceptive faculties in this type of individual were active, we had usually the amusing example, of an investigator without reasoning powers. It was a gentleman belonging to this type, who asked what "Bill Stickers" had done, that he should always be told to "beware."



The deficiency in the reasoning faculty, appeared to

produce no detrimental effect on these persons as regards their acquiring stock knowledge, such as languages, the facts connected with artillery, or experimental chemistry, or even in drawing. Many cadets possessing this form of head were capital artists, and could sketch well, even from nature. Set them a problem requiring calm reasoning, and they were at once out of their depth. Seeing a head of this form, I should feel confident that its possessor would not be able to comprehend that the moon rotated on its axis, or that the earth could possibly have two rotations.

Another form of head that exhibited a marked individuality, was the low, retreating forehead, combined with great height and breadth above and behind the ears. This form is shown below. A large amount of self-esteem, carried to the extent of conceit, a firmness exhibited mainly in obstinacy, and a desire to gain praise, under even false pretences, whilst the reasoning powers were weak, were the characteristics of this individual. Whenever such a form of head came under my instruction, I found it necessary to use additional vigilance, to prevent this cadet from fudging, either at an examination, or during the half-year.



Having studied these forms of head, I would undertake to frame an examination paper, not one question of which could be answered by the last two individuals named, and I could with tolerable accuracy sketch the

forms of heads of those gentlemen, who were kind enough to pronounce my astronomical problem worthless, because I had stated that the semi-axis of the earth traced *a circle* in the heavens during one conical movement of this semi-axis.

Hereditary genius was a question which I fear I could not corroborate from the experience I gained. The originators of this theory have given evidence of the number of men who have become judges, when their fathers, or uncles, have been judges before them. It is, however, possible that interest, introductions, and influence, may have had something to do with these results. I am surprised, however, how little advantage has been taken of the evidence afforded by the army, in favour of hereditary genius. A general officer, for example, may be appointed to a command; his name may be *xy*. In the majority of cases we shall find that, out of the whole army, the officer most fitted for the appointment of aide-de-camp, or military secretary, is also named *xy*. If not named *xy*, he may be named *z*, and in either case he is a near relation of the general's. If the theory of hereditary genius is to be proved by the fact that sons of judges become judges, or the sons of bishops become bishops, the army gives far more numerous evidences of similar results in favour of the theory.

I have been led to the conclusion that, to obtain the best health, it is as necessary to exercise the mind as it is the body. In fact, I believe it is more

detrimental to the state of one's general health, to allow the mind to remain dormant, than it is to avoid bodily exercise.

My duties at the Academy were sometimes what might be termed trying. Three mornings during the week it was necessary that I should be in Academy at eight o'clock, and ready at once to give a lecture, or to set problems and answer questions; the attendance lasted three hours or five hours, according as the attendance was a short one, or a "long morning." Probably I had been engaged on the previous night till twelve o'clock, or even later, at the Observatory with a class of officers. In order to be in the Academy, and ready for work by eight a.m., it was necessary to be called at half-past six, so that there was not more than six hours' rest.

By careful attention to diet and other sanitary matters, I can claim what our American cousins term, a singular record, viz., that during the fifteen years that I was employed at the Academy, I was never absent even a day from my work, in consequence of sickness. I attribute this result, not to any remarkably healthy constitution, but mainly to the fact, that the mind was exercised equally with the body; and in fact, that the exercise of the brain kept the body healthy.

I am so convinced that health is to be obtained as much by exercising the brain, as by exercising the body, that I believe many individuals practically commit suicide, or at least become confirmed invalids, merely

because they allow their brains to degenerate into a condition very nearly approaching imbecility.

Whilst, however, we exercise the brain, and obtain health thereby, it is very easy to overwork it, and produce disease or ill-health thereby. It is the same with the body: exercise is one thing, overwork is the abuse of exercise.

Frequently, when working at various problems, some of which have been described in these pages, I have found that my ideas would become confused. I began to doubt whether I had correctly spelt the most simple words. I would add up a row of figures two or three times, and make the sum different each time. The hint was at once taken. Brain-power, expended for the time, was the conclusion, and recreation was at once sought. A walk with a companion, even a dog, a game of billiards, or anything to divert the mind, and in a few hours the brain-power had returned. To persevere in working the brain, when it is incapable from exhaustion, of acting in a healthy manner, is almost suicidal.

What can be a more pitiable sight than to see young men, sitting with wet cloths round their heads, cramming for a competitive examination, and fancying that by so doing they are strengthening their mental powers? They might as well sit with a wet cloth round their bodies whilst they crammed beef-steaks down their throats, under the belief that by so doing they strengthened their bodies for an athletic competition in which they were about to take part.

The result of this mental cramming produces men, not with clear reasoning minds, but with crammed brains, incapable of forming a correct judgment on subjects which have not been stuffed into their mental storehouse. We have seen many examples of this diseased class of mind, as shown by the objections, and arguments of persons, against the first rotation of the earth when brought forward several centuries ago, and exactly similar objections brought against the second rotation when I first made it known.

Just as the body must digest food, to be of use to the whole frame, so must the brain digest acquired knowledge. Take any crammed youth, who has passed an examination successfully, and some five years afterwards, test how much has been retained in his brain, of the knowledge which he crammed. Usually the deposit remaining will be nothing, or, at least, so slight, that he will not be able to apply to any practical purpose that which he forced on his brain years before.

Many years ago I was riding on the box seat of a coach, when the driver called my attention to one of his horses. "That horse, sir," he said, "we can't tire. He has a lot of work in him; and I'll tell you why. He was an artillery horse; he didn't do much work, but he had such a lot of hard food, which he has turned into muscle, that we can't get to the end of it."

There was wisdom in this remark. Apply it to the brain, and we obtain similar results—useful facts stored up, technical knowledge at one's fingers' ends, should

be the contents of our mental knapsack. The brain *strengthened* by reasoning and reflecting, and we have a man fitted for most things. Cram the brain to repletion with a mass of stock knowledge, never give time for reflection or reasoning, and the brain vomits this, or retains it as a detriment to its reasonable use.

When I first took up my duties at the Academy, the pay of the Instructors was given in a very complicated way. Regimental pay according to rank was given, lodging money according to rank, and pay for the appointments varying from five to ten shillings a day. Thus it was cheaper to employ a subaltern than a captain, whilst a lieut.-colonel was a very expensive Professor. It happened that a lieut.-colonel of Engineers had been employed as a Professor some years before I joined the Academy. This officer drew the pay and half-pay of his rank, as an employed officer; he was allowed a house, and received the pay for being Professor. This accumulation of pay amounted to a large sum, so it occurred to the authorities, that the only possible way of avoiding this expensive item was to obtain an Order in Council to the effect, that no lieut.-colonel was in future to hold any appointment at the educational establishments.

On weighing carefully my rate of promotion, I found I should obtain the position of Professor some few months before I became a lieut.-colonel, consequently I should hold the more lucrative appointment only a few months, according to order. The question was how to avoid this result.

Having carefully considered the case, I pointed out to the authorities, that to draw my regimental pay from the agents, my lodging allowance from the barrack master, and my pay as Instructor from the Academy, was a very complicated way of doing finance business. The authorities saw this at once, and when I suggested that the simple remedy was to give to a Professor, irrespective of his rank, a certain annual income, and to an Instructor an annual income, the suggestion was at once adopted, and made a rule.

Having obtained this alteration, I saw my way to the next. I pointed out that the Order in Council which forbid a lieut.-colonel from holding an appointment, had been framed, merely to avoid the expense formerly incurred by the system of payment. When the pay was consolidated, not only was there no extra expense, but a saving was effected, by employing a lieut.-colonel, as he received no more pay at the Academy in consequence of being a lieut.-colonel, and his regimental pay was saved; whereas if a captain were appointed as Professor, only a captain's pay would be saved. To obtain the alteration of an Order in Council is a matter of some difficulty, but the then Minister of War, Sir John Pakington, was a reasonable, clear-sighted man, and when I had the matter placed before him, he at once saw the advantage of keeping a lieut.-colonel at the Academy, as he cost no more, but really something less than a captain; consequently he obtained an alteration in the Order in Council,

and I retained my appointment as Professor on obtaining the rank of lieut.-colonel.

For such an appointment as Professor at the Academy there were a multitude of candidates, hungry for a good appointment. As I knew of the alteration in the rule, some weeks before it was officially announced, I was amused at the endeavours of these candidates, to push themselves into notice, and the certainty they assumed of my having to leave the Academy, on promotion to lieut.-colonel.

The rule, as it formerly existed, was no doubt sound, but when the pay became consolidated, to continue it would have been unreasonable. To obtain the desired result, the first move was to get the pay consolidated. Having accomplished this, the next step was merely an act of common sense, which followed as a sequence.

CHAPTER XXIV.

Sir Frederick Roberts—Blind Rules in India—H.R.H. the Duke of Connaught—Career of Officers.

I HAVE already stated that several officers who have since distinguished themselves, came to me during the period that I held my appointment, to be instructed in surveying and practical astronomy. None of these worked harder, or showed more talent, than one who, I trust, should he read these experiences, will pardon me for mentioning his name. This was Sir Frederick Roberts. Every detail connected with sketching, reconnaissance, and practical astronomy, he worked at as though preparing for an examination. His object, however, was merely to obtain a knowledge of these subjects, to be stored, and used when occasion required.

In the year 1876, I, as lieut.-colonel, took the old 21st Brigade of Artillery to India, and commanded it till I left India in 1878. Sir Frederick Roberts was then Quartermaster-General in India, and it having been considered desirable, that alterations should be made, both in the details, and rearmament, of various forts in the Bengal Presidency, I was appointed president of the committee

ordered to assemble for this purpose. Allahabad was the first fort I inspected and reported on; after this I visited Agra, Gwalior, Delhi, and Fort William, Calcutta. The suggestions I made, were all carried out, and I have reason to believe that I saved the Indian Government many lacs of rupees.

I have now to relate an incident which will illustrate the effects of routine as carried out in India.

The hot weather having set in, during which my committee was not required to meet, I returned to my station at Morar, and resumed command of my brigade. Having been specially employed by the Indian Government during the time I was on these committees, I was allowed to draw my brigade command pay.

Shortly after resuming command of my brigade, I received a private letter from the Deputy Adjutant-General, Royal Artillery, stating that I was to be appointed as senior member of a committee to consider the defences of Aden, and that this committee was to meet shortly at Simla. He added that he wanted to talk the matter over with me, and wished I could come at once to Simla. That if I did not mind paying my own expenses to Simla, he would like to see me at once, so if I obtained leave, this leave would be cancelled immediately the committee assembled, and from the date of the first meeting of the committee I should draw my brigade command pay.

Had I waited to be ordered to Simla, the expenses of my journey, some thirty pounds, would have been paid by

the Indian Government. Being desirous, however, of giving all the aid in my power, I applied for leave; and believing that, as my leave would be cancelled on the meeting of the committee, it did not signify how much leave I applied for, I asked for six months.

A few days after arriving at Simla, I commenced my duties on the Aden committee, and continued there during seven months.

Previous to the committee breaking up, each officer on it was recommended for five rupees a day, to cover the extra expenses at that station. It was suggested, however, that I should not claim this, as I was drawing my brigade command pay, and I at once assented.

Each month I sent the usual certificates required to enable me to draw my whole pay, and heard no objections until just as I was leaving India, when it was announced to me that 2,400 rupees were disallowed from my command pay.

On inquiry I found that there was a regulation, to the effect that, if an officer who was on leave was appointed on a court-martial, or any other duty, he was not to draw his command pay until his leave had expired.

Remonstrance was useless; it was admitted that I was specially selected for the Aden committee. It was also admitted that had I resigned my leave, and returned to Morar, and done one day's duty, and had then, as I should have done, received orders to proceed to Morar, I should not only have received all

my command pay, but the Indian Government would have paid the expenses of my journey to Simla.

Finding appeal useless, I then applied for the five rupees a day, which each officer on this committee had been granted. I was met with the objection, that I was the only officer on the committee who had not been recommended for it, and therefore it could not be granted.

I stated that the reason why I had agreed to withdraw my claim to the five rupees a day, was because it was considered settled, that I was drawing command pay. The official objector stated, he knew nothing about this; all he knew was, that I had not been recommended for five rupees a day, as every other officer on the committee had been, and therefore it could not be sanctioned.

It occupied me nearly two years, with volumes of correspondence, to obtain even this small sum, and it was then granted, not as an act of justice, but with the intimation that it was not to be taken as a precedent.

I only trust, for the credit of common sense, it may not be taken as a precedent.

For my labours on the various committees, and for the sums saved the Indian Government, I received through the Commander-in-Chief the thanks of the Government in Council, and was, by a technicality, deprived of the sum of 2,400 rupees!

These facts may be of value to officers who may be similarly situated.

His Royal Highness the Duke of Connaught, then Prince Arthur, joined the Academy as a cadet, during the time that I was Professor. Our instructions were that he was to be treated just as was every other cadet, and these orders were rigidly carried out. In consequence of His Royal Highness having a thorough knowledge of French and German, it was not necessary that he should attend classes for instruction in those subjects. The more practical portion of the Academy course was therefore allotted to him, and the Professors were employed to give separate instruction to him during the attendances, at which the other cadets learnt French, and German. This arrangement did not in any way interfere with the other duties on which the Professors were employed, as the attendances were extras.

It would be out of place for me to offer many remarks as regards the application, and talent, shown by His Royal Highness, whilst under my instruction; I may, however, state that, had he been required to compete with the other cadets of his term, in an examination on the subjects in my branch, he would have come out very much nearer the top, than the middle of the class. I have formed this conclusion, not from theory, but from the results of a separate examination before the then Council of Military Education.

The Prince Imperial was also a cadet under my instruction, and I was early impressed with the manner, in which he concentrated all his energies, on the subjects he was at the time studying.

His remarks were often singularly clear and original, and proved that he reasoned, and was not content merely to cram.

On one occasion the Prince was drawing a contoured plan of ground, and I called him up in order to test whether he understood his work. In answer to my question, he replied that he had it all in his head, but could not clearly put it into words. Wishing to test this assertion, I marked the position of a battery on his plan, and made three small marks on different parts of the sketch, and asked him if the battery could be seen from these three marks. The Prince quietly examined the sketch, and deliberated for a few minutes, and then replied: "From that point the battery would be seen; from those two points, no." His replies were correct, and convinced me that he understood what he was about.

The Prince had the makings of an admirable soldier, and he was a thorough gunner, devoted to everything appertaining to artillery. His fate is too painful a subject to refer to again. Young, daring, venturesome, and brave to a fault, he was not aware of the cunning of the enemy in whose country he had ventured. But in this respect he was not singular; officers in high positions displayed similar want of knowledge. The disaster

at Isandula was repeated when the Prince was slain, and from the same cause, viz., by want of proper precaution against the special warlike system of the enemy against whom we were carrying on war.

It has been a subject of great interest to me, to watch the career of various officers who when cadets were under my instruction. I have noted how some of these have risen rapidly in rank, and occupied important positions, not, as I could discover, from any special talent, or application on their part, but because they have been carried on the express train of interest. Others, having far superior mental qualities, have struggled on, doing their duty ably in every position in which they have been placed, but have remained unnoticed and unselected, eventually quitting the service, either in disgust, or on account of age; and this compulsory retirement for age, occurring when they were in full vigour, in sound health, with constitutions hardened, and frames seasoned, to endure any climate and any hardship.

A strange career is that of the army, where what some term luck is so large an element in the conditions affecting success; as an example of which I cannot quote a more remarkable case than one of which I was myself a witness years ago, where a captain of artillery, who had fought at Waterloo, marched past on parade, and had to salute, a brevet-major of artillery, who had not been born when his junior in rank, had served in the Peninsula.

The return to regimental duty after fifteen years'

employment as Instructor, and Professor, was not without its attractions. To visit India had always been my wish, and the opportunities which my position of President of Committees gave me of seeing various interesting places, was a great advantage. From India I was ordered to Nova Scotia; rather a change of climate, but the vastness of the country, its climate, sports, and people, rendered my final active service, during four and a half years, not the least enjoyable of a somewhat varied military career.

THE END.

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*Davidson's Smaller Solid Models, in Box, £2, containing—

2 Square Slabs.	Octagon Prism.	Triangular Prism.
Oblong Blocks (steps).	Cylinder.	Pyramid, Equilateral.
Cubes.	Cone.	Pyramid, Isosceles.
Square Blocks.	Jointed Cross.	Square Block.

SOLID MODELS, &c.—*Continued.*

*Davidson's Advanced Drawing Models, £9.—The following is a brief description of the Models:—An Obelisk—composed of 2 Octagonal Slabs, 26 and 20 inches across, and each 3 inches high; 1 Cube, 12 inches edge; 1 Monolith (forming the body of the obelisk) 3 feet high; 1 Pyramid, 6 inches base; the complete object is thus nearly 5 feet high. A Market Cross—composed of 3 Slabs, 24, 18, and 12 inches across, and each 3 inches high; 1 Upright, 3 feet high; 2 Cross Arms, united by mortise and tenon joints; complete height, 3 feet 9 inches. A Step-Ladder, 23 inches high. A Kitchen Table, 14½ inches high. A Chair to correspond. A Four-legged Stool, with projecting top and cross rails, height 14 inches. A Tub, with handles and projecting hoops, and the divisions between the staves plainly marked. A strong Trestle, 18 inches high. A Hollow Cylinder, 9 inches in diameter, and 12 inches long, divided lengthwise. A Hollow Sphere, 9 inches in diameter, divided into semi-spheres, one of which is again divided into quarters; the semi-sphere, when placed on the cylinder, gives the form and principles of shading a dome, whilst one of the quarters placed on half the cylinder forms a niche.

*Davidson's Apparatus for Teaching Practical Geometry (22 models), £5.

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*Slip, two set squares and T square, 5s.

*Parkes's Case of Instruments, containing 6-inch compasses with pen and pencil leg, 5s.

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* Models, &c., entered as sets, can only be supplied in sets.

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